Imperial Bureau of Plant Breeding and Genetics

The New Genetics in the Soviet Union

by

P. S. HUDSON and R. H. RICHENS

School of Agriculture
Cambridge
England

CONTENTS

I. INTRODUCTION

Much confusion as to the present state of genetical research in the U.S.S.R. is evident in the minds of scientists from other countries. Until 1930, there were few differences to be observed between the types of genetical theory held in Russia and those accepted outside, but since that date marked divergences have appeared. The history of science, in general, has exhibited a marked coherence in the unity of scientific ideas, national and cultural boundaries exerting only a minor influence. But today this is not so in the sphere of genetics, where a school of thought has arisen which differs profoundly from the rest of the scientific world in its vocabulary, conceptual system and general outlook. This school of thought is constituted by the followers of the Ukrainian botanist Lysenko, and appears to be largely confined to those countries which lie in the sphere of influence of the Soviet Union.

It is not possible to say what proportion of Russian geneticists are in sympathy with this school. Its main adherents appear to be plant breeders and agriculturalists, and there is evidence that it is regarded coldly by many Russian biologists. Nevertheless, it occupies a very prominent place in Russian scientific literature and has increased markedly in numerical strength during the last decade. Whether or not it is provoking a reaction is uncertain, and the extreme difficulty of assessing accurately the general opinion of Russian geneticists makes any prediction as to its future development extremely hazardous.

There was, of course, a well-known school of Russian Mendelian geneticists which flourished during the second and third decades of the present century. It included such eminent biologists as Filipčenko, Vavilov, Navašin, Dubinin, Serebrovskiĭ, Dobzhansky and Timofeev-Ressovskiĭ, of whom the last four are still alive, although only Dubinin, Navašin and Serebrovskiĭ have stayed in Russia continuously. Timofeev-Ressovskiĭ returned from Germany to Russia recently. Not long after 1930, however, a new school of genetics arose, headed by Lysenko, which was antipathetic in many ways to the older school. After ten years of acute controversy, the school of Lysenko appears to have gained a position of ascendancy and the Mendelian point of view is now far less often expressed than formerly. It is the object of this bulletin to describe the principal tenets of Lysenko's school and to determine as far as possible their validity.

Up to the present few attempts have been made to approach the Russian non-Mendelian school of genetics impartially. Yet this is essential if any use is to be made by western scientists of the voluminous output of published articles by Russian investigators. A great deal of experimental material and a welter of scientific speculation has been published in Russian scientific periodicals, which, owing to the marked divergence of views betwen Russian geneticists and those outside, has not been assimilated into the corpus of genetical thought in other countries.

A few attempts have been made to review the achievements of Lysenko's school during the past fifteen years, but in general these have not been analytical, and have, moreover, been often marred by the expression of strong sentiments either for or against the Russian situation. This emotional reaction on the part of western scientists has only served to increase the difficulties of an impartial appraisal. On the one hand, extravagant praise is meted out to Lysenko and his followers, while on the other, feelings approaching panic, anger, or an attitude of extreme reserve are encountered. No adequate attempt to examine the Russian claims has been made nor any effort to subject the current theories to a stringent logical analysis. This bulletin has been written with the intention of evaluating the contributions of the Russian non-Mendelian genetical school, and of describing for the benefit of English readers its characteristic tenets. It is based, moreover, almost entirely on the original Russian scientific literature, a knowledge of which is essential for the understanding of the many unfamiliar concepts of Lysenko's genetical system. Only slight attention will be paid to the writings of authors with Mendelian views, since there are numerous texts which deal adequately with this point of view.

Haldane[b,c] is one of the few English geneticists who has given attention to these recent developments in the U.S.S.R., and his attitude has been appreciative on the whole, at least with regard to the fundamentals of the Russian outlook, although he has deprecated some of the attacks made by Lysenko on Mendelian genetics. He has, moreover, suggested that recent work on directed mutation lends some support to several of Lysenko's claims. Other English writers dealing with the subject include 'Espinasse,[a] who reports the Russian genetical conference of 1939, Polanyi, who has expressed concern at the influence of political pressure on

Russian scientific affairs, and Huxley, who was a member of the delegation of scientists present at the 220th anniversary celebrations of the Academy of Sciences of the U.S.S.R.

In America, Pincus has reported the 1939 conference, without, however, expressing a decided opinion on the issues involved, while Sax[a,b] has condemned strongly what he believes to be the subservience of Russian genetics to the official political philosophy, and also the Lamarckian complexion of several of Lysenko's theories. An uncritical reply to Polanyi's criticisms was published by Kartman in 1945.

Kesselring and Oettingen, in Germany, have both written appreciatively on the plant-breeding achievements of Mičurin, but they point out that the principles that he employed need further investigation. The Bulgarian writer Plotikanov and the Polish geneticist Lewicki both emphasize the need for caution in assessing the value of Lysenko's claims; they point out that a confirmation of many of the experimental data is desirable, also that Lysenko's anti-Mendelian views are not always adequately substantiated. In Portugal, Câmara[a,b] has criticized the Lamarckian traits of Lysenko's system and has shown that his criticisms of Mendelian genetics are sometimes based on misconceptions.

The problem of coming to a satisfactory conclusion as to the merits and demerits of the modern Russian school admits of no easy solution. Purely scientific matters are so entangled in philosophical, historical and psychological issues that an investigation confined to the Russian facts and theories alone would shed but a feeble light on the system as a whole. Consequently, the unusual course has been adopted in this bulletin of prefacing the detailed analyses of the published data by a general account of the historical and psychological background of Lysenko's school. It can be confidently claimed that, without this knowledge, the Russian outlook can never be understood or appreciated. Much that appears meaningless or even absurd to western readers unacquainted with the historical origins of the system, becomes plain, or at least, less obscure, when viewed in a wider perspective. This, it is believed, fully justifies such a procedure, since it is futile to try and evaluate a system if its fundamental concepts are not adequately comprehended.

It is hardly necessary to point out here the many services that Russian geneticists have already rendered to world genetics. Vavilov's phytogeographical researches on the origins of crop plants, Mičurin's pioneer work on distant hybridization and Lysenko's studies on vernalization are contributions to biology which transcend any controversial issues. If little attention is paid in this bulletin to such achievements, it is because the object of the bulletin is not to deal with progress in genetics generally, but merely to elucidate one particular controversial aspect of it. But it is not claimed that this end has been more than approached. The extreme difficulty of ascertaining the real facts, and the uncertainty as to whether the views expressed by some Russian geneticists do in fact correspond to the notions believed and employed in practice, make it impossible to present an adequate review. Yet if some of the underlying issues have been clarified, some progress at least will have been made towards elucidating a very complex problem.

References will be found at the end of the bulletin arranged alphabetically and differentiated where necessary by means of index letters.

II. HISTORY

The genetical school of Lysenko has arisen from the conflux of several distinct schools of thought, the more important being the system of dialectical materialism, the school of Soviet Darwinism, the schools of plant breeding founded by Mičurin in Russia and Burbank in the U.S.A. and the Mendelian school which dominates genetical thought outside the Soviet Union. The interaction between these various schools of thought in Russia has been very complex and at times violent. There are signs also that the present position is only temporary, and that within the next ten years further fundamental changes will occur.

In this chapter these various components of modern Russian genetics will be treated separately and their interactions described. All considerations as to the relative validity of the systems will be postponed until Chapters IV, V and VI.

Dialectical Materialism

Marx and Engels, the founders of dialectical materialism, indicate quite clearly its historical origins. On the one hand, it derives from the so-called "vulgar" materialism of eighteenth

century France and nineteenth century Germany, and on the other, from the idealism of Hegel.

Materialism had appeared at intervals in the history of philosophy from the time of the Greeks onwards. Its chief protagonists in early times were the Greek philosophers Democritus and Epicurus and the Roman poet Lucretius. In their writings, the essence of the later systems appears, viz. the denial of immaterial existence and the claim that the properties of bodies can be adequately explained by the movement of material particles or atoms. This view naturally entails a belief in the eternity of matter. During the Middle Ages, philosophies based on Platonic and Aristotelean principles became dominant and atomistic systems fell into disfavour, the only important exception being the writings of Nicholaus de Autricuria.

At the Renaissance, atomism was revived by Gassendi. Descartes established his system of metaphysical dualism, whereby matter and mind were divorced one from the other; and these two philosophers together with the English empiricists and the French sceptics, paved the way for the French materialists of the eighteenth century, such as La Mettrie, Diderot, Helvetius and Holbach. The German materialists of the early nineteenth century were influenced to some extent by the French school and also by the reaction against post-Kantian idealism which took place towards the latter end of the life time of Hegel. Feuerbach was one of the foremost of these and his influence on Marx and Engels is quite evident.

The principal tenets of the older materialists that were incorporated into the dialectical materialism of Marx and Engels were the denial of immateriality and the affirmation of the extra-mental existence of the real world, the latter tenet being common to all realist philosophies as well. Implicit in the older materialism, however, was a denial of the possibility of progress or change in any fundamental sense. Apparent change was explained as arising from the fortuitous concourse of atoms in motion, which were believed to have undergone combination and recombination from all eternity, and would so continue forever. There was thus little place for an appreciation of such temporal events as historical development with all its sociological implications.

Marx attempted to rehabilitate history by grafting on to the older materialism of the eighteenth and nineteenth centuries, a modification of the notion of the dialectic developed by Hegel. In its origins, this notion is Platonic and is used in Plato's dialogues to cover the development and clarification of ideas that comes about in the course of discussion. Hegel extended this signification to cover the development of ideas in history, which movement he regarded as expressing the evolution in time of the Absolute Idea, which for him was the ground of existence.

Marx re-interpreted this notion by making the dialectic, not a property of the Idea, but of matter. In this way, change and development and in particular historical development, became significant elements in his materialism, and the old closed system of eternal reiteration was broken.

Throughout his life Marx was ably assisted by Engels,[a,b] who took considerable pains in an attempt to demonstrate the workings of the dialectic in historical, biological and physical processes. Dialectical materialism was further developed by Plehanov and Lenin, and after the October Revolution in Russia became the official philosophy of the Soviet Union.

It is difficult to determine how far Lenin modified the original philosophy of Marx and Engels. There is evidence that he was influenced by the vulgar materialism of the Russian nihilists Pisarev, Černiševskiĭ, Dobroljubov and Nečaev and the revolutionary theories of Tkačev. The former writers, who acknowledged their indebtedness to the German materialists Moleschott, Vogt and especially Büchner, exhibit something of the dogmatic materialism characteristic of later Soviet thought, and this aspect of Russian Marxism may derive more from the nihilism of Imperial Russia than from the dialectical materialism of Marx and Engels. It is noteworthy that the revisionist movement that developed amongst Marxist thinkers in the early years of the present century showed a far greater appreciation of the merits of other philosophical systems than do the present exponents of Soviet Marxism. Lenin wrote constantly against the former thinkers, insisting strongly on the need for maintaining unaltered the system of revolutionary Marxism which he himself had done so much to establish.

Lenin's book, *Dialectical Materialism and Empiriocriticism*, made an important contribution to the elucidation of the relationship between dialectical materialism and the natural sciences, and his methods of discussion have been widely adopted by later Russian writers. It only remains to mention Stalin, whose numerous writings have an important bearing on the development of Russian science. He analyses the attitude of mind that a scientist should show towards the philosophy of dialectical materialism, and indicates the proper fields of inquiry for the Marxist scientist.

Soviet Darwinism

Owing to an unfortunate development in terminology, the term "Darwinism" has come to have a very different connotation in the Soviet Union from that which it bears in Western Europe. In most European countries, Darwinism is understood to apply to the theory of evolution proposed by Darwin, in which natural selection plays an important role. In Russia, on the other hand, the term has been applied to the whole corpus of Darwinian hypotheses, including his partially Lamarckian conception of the causes of variation and his peculiar views on self and cross pollination. It is important therefore to bear this difference in mind when discussing the tenets of Soviet Darwinism.

From an historical point of view, it is unnecessary to trace the basic concepts of Soviet Darwinism any further back than to Lamarck. In the *Philosophie Zoologique* is to be found a clear exposition of the theory that "every fairly considerable and permanent change in the environment of each race of animals effects in it a real change in their needs"[1]; and that "every new need necessitating new actions for its satisfaction, requires of the animal which experiences it, either the more frequent use of such of its parts which it formerly used less (which develops and enlarges them considerably), or else the use of new parts which the needs cause to come into existence in it through the strivings of its inner consciousness".[2] Lamarck believed, moreover, that these acquired characters might be more than temporary modifications, and that "everything that nature has caused individuals to acquire or lose through the influence of the environment to which their race has long been subjected (and consequently through the effect of the prevalent use of a certain organ, or through a permanent disuse of a certain part) she transfers to the new individuals which arise by reproduction, provided that the acquired changes are common to the two sexes, or to those individuals which produce the offspring".[3]

These speculations did not win general acceptance at the time and it was only with Darwin's[a,b,c] publications, that evolutionary theories became part of general biological theory.

It is very important to examine Darwin's views carefully since a large proportion of the characteristic tenets of Lysenko's school are to be found, at least in embryo, amongst Darwin's writings. In the *Origin of Species* Darwin presented his evidences for the belief that species arose by divergent evolution from a few primitive forms, only those types surviving whose characteristics enabled them to survive the operation of natural selection. He believed that organisms were capable of varying within certain limits and that these variations were heritable and were selected in accordance with their adaptability to the conditions under which the organisms flourished.

Regarding the causes of variation, Darwin was in considerable doubt, and his views appear to have fluctuated to some extent. He remarks in the *Origin of Species* that "some have even imagined that natural selection induces variability, whereas it implies only the preservation of such variations as arise and are beneficial to the being under its conditions of life", and he confirms his belief in the negative role of natural selection later, remarking that "natural selection acts exclusively by the preservation and accumulation of variations, which are beneficial under the organic and inorganic conditions to which each creature is exposed at all periods of life". Yet he concedes that "variability is generally related to the conditions of life to which each species has been exposed during several successive generations". It is suggested also that "adaptation to any special climate may be looked at as a quality readily grafted on an innate wide flexibility of constitution, common to most animals", and that, although "changed conditions generally induce mere fluctuating variability . . . sometimes they cause direct and definite effects".

Theories on the relationship between variation and environment were developed further by Darwin in his book on *The Domestication of Plants and Animals*. He maintains as before that it is "probable that variability of every kind is directly or indirectly caused by changed conditions of life", and conversely, that, "if it were possible to expose all the individuals of a species

---

[1] Tout changement un peu considérable et ensuite maintenu dans les circonstances où se trouve chaque race d'animaux opère en elle un changement réel dans leur besoins.

[2] Tout nouveau besoin nécessitant de nouvelles actions pour y satisfaire, exige de l'animal qui l'éprouve, soit l'emploi plus fréquent de telle de ses parties dont auparavant il faisait moins d'usage, ce qui la développe et l'agrandit considérablement, soit l'emploi de nouvelles parties que les besoins font naître insensiblement en lui par des efforts de son sentiment intérieur.

[3] Tout ce que la nature a fait acquérir ou perdre aux individus par l'influence des circonstances où leur race se trouve depuis longtemps exposée, et par conséquent, par l'influence de l'emploi prédominant de tel organe, ou par celle d'un défaut constant d'usage de telle partie; elle le conserve par la génération aux nouveaux individus qui en proviennent, pourvu que les changements acquis soient communs aux deux sexes, ou à ceux qui ont produit ces nouveaux individus.

during many generations to absolutely uniform conditions of life, there would be no variability". Not only large alterations in the environment are envisaged in this connexion, for he remarks later that "changes of any kind in the conditions of life, even extremely slight changes, often suffice to cause variability".

Yet Darwin by no means felt satisfied that the environment could be invoked to cover all cases of variability. When dealing with bud mutation of fruit trees, he confesses that he "cannot imagine a class of facts better adapted to force on our minds the conviction that what we call the external conditions of life are in many cases quite insignificant in relation to any particular variation, in comparison with the organisation or constitution of the being which varies". He amplifies this later by admitting that he had "become deeply impressed with the conviction that in such cases the nature of the variation depends not on the conditions to which the plant has been exposed, and not in any especial manner on its individual character, but much more on the inherited nature or constitution of the whole group of allied beings to which the plant in question belongs".

As to the precise way in which the environment affects variability, Darwin could only make tentative suggestions. He inclined to the opinion that "of all the causes which induce variability, excess of food, whether or not changed in nature, is probably the most powerful". This opinion he attributes to the plant breeder Andrew Knight, and he also mentions the possibility that unnatural conditions may of themselves cause variation to arise.

Two other quite different causes of variation are also suggested in Darwin's writings. There is to be found firstly the theory of Use and Disuse which Darwin borrowed from Lamarck. Without attempting to give an adequate explanation why such should be the case, Darwin puts forward his opinion that "there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited". Secondly, Darwin believed that the tendency to atavistic reversion was latent in organisms and that many cases of variation were "certainly due to reversion to characters not acquired from a cross, but which were formerly present and have since been lost for a longer or shorter time". Various environmental conditions and the effect of hybridization were thought to favour the expression of this tendency.

Summarizing at this point, it can be said that Darwin definitely believed that the environment could cause a directional change in the hereditary constitution of organisms, and that the Lamarckian principle of Use and Disuse and atavistic reversion were also operative in this respect. It should be emphasized, however, that Darwin was far from satisfied that these three causes were sufficient to cover the whole field of genetical variability. He is constantly acknowledging the incompleteness of biological knowledge concerning these matters, and he is still more tentative when allowing himself to conjecture on the mode in which these factors exerted their influence.

It is important also to consider Darwin's views on fertilization, which are mainly to be found in his book on *The Effects of Cross and Self Fertilisation of Plants*, and which he derived in part from the plant breeders Sprengel and Andrew Knight. In the *Origin of Species* he had summarized his views on this subject by stating that he had "collected so large a body of facts, and made so many experiments, showing, in accordance with the almost universal belief of breeders, that with animals and plants a cross between different varieties, or between individuals of the same variety but of another strain, gives vigour and fertility to the offspring; and on the other hand that close interbreeding diminishes vigour and fertility; that these facts alone inclined him to believe that it is a general law of nature that no organic being fertilises itself for a perpetuity of generations; but that a cross with another individual is occasionally—perhaps at long intervals of time—indispensable". A proviso is mentioned later when he declares that the problem of "whether long-continued self-fertilisation is injurious to all plants is another and difficult question".

In attempting to account for these generalizations, Darwin falls back upon his theory that the environment conditions the nature of variability. He states that "two plants which resemble each other as closely as the individuals of the same species ever do, profit in the plainest manner when intercrossed, if their progenitors have been exposed during several generations to different conditions". This he reaffirms later by explaining that "the advantages of cross-fertilisation do not follow from some mysterious virtue in the mere union of two distinct individuals, but from such individuals having been subjected during previous generations to different conditions, or to their having varied in a manner commonly called spontaneous, so that in either case their sexual elements have been in some degree differentiated. And secondly . . . the injury from self-fertilisation follows from the want of such differentiation in the sexual elements".

Darwin appears to have convinced himself that "plants which have been propagated for some generations under different climates or at different seasons of the year transmit different constitutions to their seedlings", but he admits that the reason why cross-fertilization between such differentiated plants should be beneficial is obscure. He does point out, however, the analogy between cross-fertilization of differentiated gametes and the affinity of dissimilar chemical substances, remarking that "it is not known why a certain amount of differentiation is necessary or favourable to the chemical affinity or union of two substances, any more than for the fertilisation or union of two organisms". It is possible to see here also that Darwin, after assembling his facts and developing his generalizations, yet remains agnostic as to the underlying causes, making only tentative suggestions as to the possible fundamental explanation.

Only one other aspect of Darwin's work need be mentioned here and that is his attitude to the problem of graft hybridization. He records various cases in which shoots developed from grafted trees have exhibited the characters of both stock and scion, either blended together uniformly or disposed in a mosaic of the parental types. He mentions that such shoots are sometimes capable of bearing progenies which segregate in respect of their morphological characters and he supposes that they are in fact true hybrids. In an interesting discussion as to the nature of sexual and vegetative reproduction, he comes to the conclusion that the two are not sharply distinguishable, and he brings forward the phenomena of xenia (metaxenia), telegony (the supposed effect of a first copulating male on the offspring obtained from an animal by a second) and graft hybridization as connecting links.

The subsequent intricate history of Darwinian notions on the continent during the latter half of the nineteenth century need not be mentioned in detail. But it is necessary to refer to the ideas of several other biologists who influenced the development of Darwinian theory in Russia, the principal being the German writers Nägeli and Weismann, the English geneticist Bateson, and the French botanists Naudin and Millardet. Burbank, the American horticulturalist, will be mentioned later, after dealing with Mičurin.

The German and English biologists are important here in that they attempted to elaborate a particulate theory of heredity, in which the transmission of characters was attributed not to the organism as a whole, but to some particular parts of it. Nägeli devised the hypothesis of the idioplasm, which was regarded as the basis both of individual ontogeny and also of inheritance. His concept, although much discussed at the time, was too obscure to win general acceptance and fell into disfavour before the more lucid ideas of Weismann. The latter contributed two important new points of view to biological theory. He firstly made a sharp distinction between the hereditary germplasm and the non-reproducing soma. The former was regarded as persisting unaffected by external influences through the complete cycle of gametogenesis, fertilization, ontogeny and reproduction, while the latter was regarded as a product of the activity of the germplasm, not exerting any significant influence upon it. Secondly, he introduced a particulate concept of heredity, regarding the germplasm as in some way the resultant of the interaction of genetic elements which he termed ids. These he definitely associated later in his life with chromosomes or parts of chromosomes, thus initiating a line of thought which was to develop into the modern theory of the gene. Bateson was, of course, the writer who first introduced this term. His views will be mentioned later when dealing with Timirjazev.

The French writers Naudin and Millardet, who published their investigations in the latter half of the nineteenth century, are far less well-known than the biologists mentioned above, but their influence on the development of genetical theory in Russia has been so marked that their conclusions require special consideration.

Naudin developed what was perhaps the most complete genetical hypothesis before the rediscovery of Mendel's paper, although it is qualitative only and makes no pretence at dealing with segregation ratios. He suggested that a hybrid plant should be regarded as "an individual in which are found united two different natures, each having their own mode of life and their own τέλος which mutually counter one another and are constantly striving to dissociate themselves from one another".[1] "The hybrid, according to this hypothesis, would be a living mosaic, in which the eye would not distinguish the discordant elements as long as they remained intermingled; but, if owing to their affinities the elements of each nature should come together again and aggregate in fairly large masses, there might result parts distinguishable by the

[1] un individu où se trouvent réunies deux essences différentes, ayant chacune leur mode de végétation et leur finalité particulière, qui se contrarient mutuellement et sont sans cesse en lutte pour se dégager l'une de l'autre.

eye."[1] That mode of hybridization in which the elements derived from each parent were believed to be finely dispersed somewhat as in an emulsion was thought to be exemplified by the numerous instances of blending inheritance. The derivative type of hybrid in which the elements of each parent were regarded as collecting together—the analogy here being with the separation into two visible phases of an unstable emulsion—was believed to be illustrated by "graft hybrids" such as *Cytisus Adami* Poit., whose chimeral nature was then unknown.

By applying this hypothesis of *disjonction* to the ovules and anthers, Naudin was able to construct a theory of segregation which covered the facts then known. Those hybrid ova into which only the elements of the male parent had aggregated would give rise to offspring of the pure male type if pollinated by corresponding pollen grains. Similarly, ova containing female elements only would, after pollination with pollen grains of female type, give rise to offspring of pure female type. Ova in which *disjonction* was incomplete would be expected to give rise to offspring intermediate between the original parental types, whatever the nature of the pollinating grains. A comparable result would be expected from the fertilization of a male-type ovum by a female-type pollen grain and vice versa. It should be noted that although this theory gave a fairly satisfactory explanation to the qualitative facts of segregation, it was not applied to the quantitative data furnished by segregation ratios.

Millardet[a,b] extended Naudin's theory to cover his own experiments on *hybridation sans croisement* or *faux hybridation*. He showed that in many interspecific and intervarietal crosses of *Fragaria* L. and *Vitis* L., the F_1 hybrids sometimes resembled one or other of the parents and gave rise to F_2 plants of the pure parental type without segregation. This finding he contrasted with Gärtner's dictum that hybrids and their offspring always exhibit some traits of either parent. He was careful to point out the possibility put forward by Focke that instances of maternal identity might be attributable to pseudogamy, but he emphasized that this suggestion would not cover the cases quoted of paternal identity.

The explanation that Millardet favoured was based on Naudin's theory of *disjonction*. He suggested that instead of the elements of either parent always maintaining their identity, it was possible sometimes that "by the very act of fertilization, certain important parts of the male or female cell were neutralized, or perhaps annihilated by the opposite cell, just as two chemical substances may bring about mutual precipitation".[2] Since however it was necessary to explain those instances in which an F_1 hybrid resembling one parent gave rise to offspring with some of the characteristics of the other, Millardet postulated that the elements transmitting the hereditary characters might become latent. He insists all the same that "although the characters of the variety absorbed are sometimes, in the case of the false hybrids, simply in a latent condition, yet they can also disappear without returning".[3]

Millardet made one other contribution to this subject by his attempts to produce anatomical evidence for Naudin's theory of fine dispersion of the parental elements in blending hybrids. He found that in the vine hybrid York Madeira, from *Vitis aestivalis* Michx x *V. Labrusca* L., the stomatal types of each parent, together with intermediate forms, co-existed closely adjacent on the surfaces of the leaves.

Having now considered briefly the characteristic opinions of the progenitors of Russian Darwinism, it is time to turn to the views of the eminent Russian botanist Timirjazev, who was the principal exponent of Darwinian ideas in pre-revolutionary Russia. Although chiefly famous abroad as a plant physiologist, he was keenly interested in genetics, which he regarded primarily as the study of the physiology of development. His writings are characterized by wide erudition, and are invaluable as evidences of the state of genetical science during the early years of the present century.

Timirjazev was born in 1843 of a family in sympathy with liberal ideas. He studied at Moscow University and became recognized throughout the world as a distinguished plant physiologist. In 1864 he began publishing a series of articles in favour of Darwinian ideas and in 1890 brought out his own interpretation of genetical phenomena which he recast later after the rediscovery of Mendel's paper. He did not live on good terms with the Imperial authorities, possibly on account of his liberal sympathies; pressure was put upon him at one point to resign

---

[1] L'hybride, dans cette hypothèse, serait une mosaïque vivante, dont l'œil ne discerne pas les éléments discordants tant qu'il restent entremêlés; mais, si par suite de leurs affinités, les éléments de même espèce se rapprochent, s'agglomèrent en masses un peu considérables, il pourra en résulter des parties discernables à l'œil.

[2] par l'acte même de fécondation, certaines parties importantes de la cellule mâle ou femelle ont été neutralisées, peut-être annihilées par la cellule adverse, comme deux substances qui se précipitent mutuellement.

[3] si les caractères de l'espèce absorbée sont quelquefois, chez les faux hybrides, simplement à l'état latent, ils peuvent aussi disparaître sans retour.

his teaching post at the Petrov Academy, and, although he was the recipient of many foreign honours, he was not elected a member of the pre-revolutionary Academy of Sciences.

He commended himself to Lenin before the October Revolution by his translation of Harwood's book, *The New Earth*, and in 1917 sided with the bolsheviks in contrast to most of his colleagues. He died three years later in 1920.

When studying Timirjazev's earlier works on genetics, it is impossible not to be struck by the breadth of his views. He was a keen advocate of Darwin's ideas and was critical of Mendelian theory as it stood at the beginning of the present century, largely because he realized many of its inadequacies. Later research has of course done much to modify the original Mendelian theory and many of the criticisms raised by Timirjazev have now been satisfactorily met by later reformulations of Mendel's original genetical theory.

On the fundamental causes of variation, heredity and evolution, Timirjazev followed Darwin closely, although he never appears to have come to a definite decision on the Lamarckian aspects of Darwin's system. He examined Lamarck's own conclusions with care and showed that there were two distinct theories to be considered, firstly the hypothesis that each organism has the ability to respond adaptively to its own needs, the implication being that this faculty has something of a conative quality, and secondly the hypothesis that characters acquired in any way may be inherited. The first hypothesis, Timirjazev rejects as inconsistent with scientific notions, but on the second he is unable to come to a decision. He points out that two types of acquired characters should be recognized, viz. those believed to arise from the effects of the external environment, and those which occur through the organism's own activity or inactivity as in the Lamarckian principle of Use and Disuse. Darwin accepted both these theories as partial explanations of natural variation, but Timirjazev felt that the evidence either way was inconclusive.

Especially important in the light of later developments is Timirjazev's classification of the types of inheritance, for this has been taken over practically as it stands by Lysenko. It represents a quite fair assessment of genetical knowledge in the first decade of the present century, although quite outmoded today by subsequent genetical investigations.

Timirjazev's scheme may be represented diagrammatically as follows:—

- Simple inheritance (one parent involved)
- Complex inheritance (two parents involved)
  - Mixed inheritance (mosaics of parental characters)
  - Blending inheritance (blends of parental characters)
  - Mutually exclusive inheritance (complete dominance of one or other parent)
    - Millardetism (F_2 generation not segregating)
    - Mendelism (F_2 generation segregating)

Simple inheritance[1] is that in which only a single parent is concerned in the production of offspring. It is exemplified by asexual and vegetative reproduction, also by the phenomenon of parthenogenesis. Complex inheritance[2] is the mode of heredity found in all other cases and is characterized by the fact that two parents are involved. It is subdivided into three categories, mixed,[3] blending[4] and mutually-exclusive[5] inheritance.

Of these, mixed inheritance is the term chosen to cover those instances where various characters of both parents are found present in the hybrid in a mosaic pattern. The examples quoted include variegated flowers and piebald animals, whose parents were marked respectively by one or other of the two pigments of the progenies. The supposed graft hybrids such as *Cytisus Adami* and others, now thought to be chimaeras, were also included, the author taking over the concept of *hybridation disjointe* elaborated by Naudin. An instance of non-sexual complex inheritance was also believed to be furnished by Delage's work on xenia (metaxenia), where an influence by the pollen tube on the maternal tissue was believed to occur. Darwin's belief in telegony, however, was not regarded as based on sufficient evidence.

Blending inheritance was understood as in the modern sense, i.e. for cases in which the

[1] наследование простое. [2] наследование сложное. [3] наследование смешанное. [4] наследовапие слитное. [5] наследование взаимоисключающее.

characters of the hybrid were intermediate in various degrees between the corresponding characters of either parent. Timirjazev of course did not explain such cases as arising from the interplay of multiple genes but rather as exemplifying an independent category of heredity.

The third subdivision of complex inheritance, mutually exclusive inheritance, was the category devised to cover all cases in which the hybrid resembled in all obvious features one or other of its parents. Such instances would be interpreted today as examples of complete dominance. This type of heredity was moreover divided into two. Those instances in which the F_2 hybrid progenies failed to segregate were classified as examples of Millardetism, while instances of segregating F_2 progenies were placed in the category of Mendelism.

Millardetism, as the name suggests, was the term coined to cover those instances of *hybridation sans croisement* investigated by Millardet in which the dominance shown by one parent in the F_1 hybrids persisted without segregation in the subsequent generations. Mendelism covered the remaining cases in which segregating hybrid progenies occurred, although it is emphasized that segregation ratios other than the 3 : 1 ratio investigated by Mendel may appear. Timirjazev also points out that the Mendelian postulate that segregation and recombination of gametes occur at random was proposed first by Naudin.

Three other topics which Timirjazev considered should also be mentioned. Firstly, there is his insistence that genetics should be treated as a problem of the physiology of development, a point of view more familiar now than in the earlier years of the century. Secondly, Timirjazev was suspicious of the method of procedure common in genetics of explaining the hereditary properties of the whole organism by attempting to locate a material basis of inheritance in some constituent part. He therefore criticized the use in genetics of Nägeli's concept of the idioplasm, Weismann's theory of the ids and their supposed relation to the chromosomes, and Bateson's concept of the gene. The implication in Bateson's writings that evolutionary change was due only to recombination of pre-existing genes and not to the development of something new was regarded unfavourably, and Weismann's distinction between the germ plasm and soma was shown not to accord with the facts of botanical ontogeny. Thirdly, Timirjazev narrowed the term Darwinism to apply to Darwin's theory that selfing causes loss of vigour and crossing increased vigour, in which respect he quotes the work of Burbank on heterotic walnut crosses. In this restricted use of the term, Timirjazev set a fashion which influenced the subsequent terminology in Russia. Timirjazev also follows Darwin in his acceptance of the graft hybrid concept, another respect in which he influenced the later development of genetics in the U.S.S.R.

After Timirjazev's death no distinctive school of Russian Darwinism appears to have existed until the introduction of Lysenko's theories. Even in 1937, Prezent[e] complained that questions of Darwinism were regarded as irrelevant by geneticists such as Vavilov and his associates. Since that date, however, and owing to the controversies engendered by Lysenko and his colleagues, a strong movement has developed which entitles itself Soviet Darwinism, a development which will be considered in detail when dealing with Lysenko.

Mičurin

The distinguished Russian horticulturalist Mičurin[a] was born in 1855 of a family of small landowners interested in horticultural problems. He was educated at the Rjazan Gymnasium and prepared for the university, but being unable to enter owing to the impoverishment of his family, took a post as deputy station-master of Kozlov. In 1874 he married the daughter of a serf and was disinherited by his family; he also incurred the displeasure of the railway authorities, and was reduced to the rank of railway clerk and transferred to Rjažsk. In the following year he returned to Kozlov and started a private nursery, still working during the day at the railway.

Mičurin very early conceived the idea of attempting to improve Russian fruit trees by introducing foreign material into the country. He came under the influence first of Grellj, a Moscow horticulturalist who had developed the theory that superior foreign varieties could be acclimatized to the rigorous climate of Russia by being grafted on to hardy native stocks. Mičurin worked along these lines for some ten years, finally rejecting the method as inefficacious.

Meanwhile he had amassed a valuable collection of east Asiatic species congeneric with the cultivated fruit trees of Russia; these he had obtained by the kind offices of travellers and hunters. In 1878, Betling, a fruit grower from the Crimea, suggested to Mičurin that he should utilize his collection for the purpose of hybridization, which Mičurin accordingly did, resigning in the following year from the railway in order to devote all his time to this new line of research.

During the next ten years his work was attended with considerable success, and his fame as a plant breeder became broadcast. He was visited in 1896 by the American botanist Meyer, who introduced some of Mičurin's new varieties into Canada and the U.S.A.

About the year 1900, Mičurin decided that the hereditary nature of hybrid seedlings was profoundly affected by their environment. His own nursery was situated on a fairly rich soil, and this he concluded caused his hybrids to become adapted to mild conditions. In order, therefore, to breed for greater hardiness, he transferred his collection to another site in which environmental conditions were more severe. This principle of Mičurin's, that the environment has a profound effect on F_1 hybrid seedlings, became later one of the characteristic tenets of his school.

The first decade of the twentieth century saw Mičurin very busy both in the practical work of breeding and in the publication of papers expressing his theoretical principles. His relations with the civil and ecclesiastical authorities of the day were not cordial; he was suspected of liberal sympathies and was reputed a luke-warm adherent of the Orthodox Church. Pressure was put on him consequently to discontinue his experiments, but without any result. Negotiations with the Russian Department of Agriculture to earn State support for his work were begun, but broke down after a time, partly through the obscurantism of the State authorities and partly through Mičurin's lack of tact. In 1913, Fairchild, the American agriculturalist, offered Mičurin a good price for his entire collection on behalf of the United States Department of Agriculture, but this offer Mičurin refused. This and other attentions from foreign horticulturalists led the Russian Imperial authorities to revise their attitude to some extent, and two minor orders of honour were conferred upon him.

In 1915, Mičurin[b] elucidated his position in respect to Mendelian genetics, which he regarded as valid only under the particular condition that both parents and hybrid were grown under the same environmental conditions. He affirmed again that the environment exerted a profound effect on hybrid seedlings and pointed out that Mendelists were unwilling to acknowledge this claim. He also went further and claimed that the transmission of hereditary characters was dependent on the comparative vigour of the two parents, which in turn was affected by environmental conditions. In contrast to the prevailing Mendelian concept that new forms arose principally from recombinations of genes, Mičurin asserted that characters might be annihilated, or conversely, created *de novo* by such operations as the "intelligent capacity for adaptation of every living organism in the struggle for existence".[1]

Mičurin[c] returned to the question of Mendelism in the following year, emphasizing again that the transmission of hereditary characters depended on the vigour of the parents and that the phenomenon of dominance was subject to environmental control. In this year also he published an account of his "mentor" theory which he derived from his experiments on grafting. He claimed that if an F_1 hybrid was used as a stock on which a scion, the mentor, was grafted, it was often possible to modify the genetical constitution of the stock in the direction of that of the scion. Such characters as maturation time, fruit colour, quality and keeping capacity, and frost resistance, were thought to be modifiable by the mentor method. At first, the mentor was always the scion, but subsequently the reverse relationship was studied, that of stock on scion, and mentor effects were claimed also in this direction. Other theories developed by Mičurin include the belief that characters adapted to local conditions appear to be dominant in hybridization, and the notion that maternal parents tend to transmit their hereditary characters more readily than the paternal.

In the October Revolution, Mičurin offered his services to the bolsheviks, and in the next year was rewarded by a subsidy. His nursery was taken over by the State, and he himself was appointed director and given assistants. Mičurin does not appear to have developed any significant new theories after the Revolution, and his time was spent subsequently in consolidating the lines of research that had already been laid down. He transferred his nursery in 1921 to the grounds of a local disused monastery, where practical courses for students were provided. In 1929 a technical college was attached.

Many signs of official favour were bestowed on Mičurin after the revolution. Lenin expressed appreciation of his work and he was frequently visited by Kalinin. In 1925 the Central Government ordered the celebration of the fiftieth anniversary of the beginning of his work; he received the Order of the Red Banner, a life pension, and six years later received the Order of Lenin. In the following year the town of Kozlov was renamed Mičurinsk. Jubilee celebrations in his honour were held in 1934; he was visited by an official delegation including Vavilov and Lysenko, and in 1935 was made an honorary member of the Academy of Sciences of the U.S.S.R. Shortly after this he died.

In contrast to the honour with which the Soviet authorities held Mičurin, the Mendelian

---

[1] **разумная сила приспособляемости каждого живого организма в борьбе за существование.**

scientists in Russia held aloof and questioned the validity of Mičurin's theoretical ideas. Vavilov indeed praised his work on distant hybridization but gave no countenance to his other ideas. In 1928, however, Keller and Sluckiĭ were appointed consultants to Mičurin's research institute, and in the next year Keller defended Mičurin at some length from the hostile criticisms of Mendelian geneticists. Subsequently, Mičurin's work has been widely publicized by the Soviet authorities, and held up as a model to other geneticists. Keller, after Mičurin's death, attached himself to Lysenko's school, whose theories he eulogized in a paper published in 1944. He died next year.

Burbank

It is convenient to consider at this point the influence of Burbank, the American plant breeder, on the development of genetical theories in Russia. He is quoted with approval by Timirjazev, Mičurin and later by Lysenko and Prezent, and there are many detailed parallelisms between the characteristic tenets of Mičurin and Lysenko on the one hand, and Burbank on the other. Many of these similarities are probably due to the adoption by the Russians of Burbank's theories. Others should more likely be explained as an example of parallel but independent development of ideas.

Burbank was born in 1849 and died in 1926, publishing extensively during his long life. He turned to horticulture early in his career and became enraptured by the theories of Lamarck and Darwin, which he interpreted in somewhat anthropomorphic fashion. Regarding the question of the effect of the environment on hereditary constitution, Burbank was confident that "there is no possible room for doubt that every form of plant life existing on this earth is now being and has always been modified, more or less, by its surroundings". He goes on to explain that "it is not the duration of an environment that affects heredity; it is the amount of pressure exerted". His explanation of the fundamental cause of natural variation departs far from the normal type of scientific hypothesis; he suggests that variation occurs because "Mother Nature . . . knew that sameness, monotony, exact re-duplication over and over again would make this world the dullest point of light in the whole universe". In his belief that inbreeding was necessarily deleterious Burbank followed Darwin, and he adopted with enthusiasm Lamarck's principle of Use and Disuse.

Burbank is at pains to point out his antipathy to "metaphysical" biological theories, but does not define clearly either what he means by "metaphysical" or what is the root of his objection.

In his firm belief in the importance of the environment on genetic constitution, Burbank anticipated the later theories of Lysenko. Several of his remarks on the power of the environment to modify genetical constitution presage Lysenko's theory of "shattering", while his conclusion that "heredity is nothing but stored environment" heralds Lysenko's dictum that "hereditary constitution is as it were a concentrate of the environmental conditions assimilated by the plant organisms in a number of preceding generations".[1] His tentative hypothesis of sap hybridization may be the antecedent of Lysenko's theory of graft hybridization, and will be considered later when this theory is analysed.

Mendelism

It is customary to apply the term Mendelism to the system of genetics most generally accepted today. Its characteristic tenets are the acceptance of Mendel's particulate concept of heredity, the belief that genes, units of heredity, are situated along the length of the chromosomes, and may, moreover, become modified by the process of mutation, and a general agreement that evolution has occurred through the action of natural selection on the phenotypes determined by the gene complexes.

In the history of Mendelian genetics, Russia has played a significant role, and many Russian biologists have made important contributions to the general theory. The two most outstanding figures under the Imperial regime were Filipčenko and Vavilov, whose early work was published extensively in English, French and German periodicals. Filipčenko was made Director of the Bureau of Genetics of the Academy of Sciences after the October Revolution. After his death he was succeeded by Vavilov, the Bureau having been reconstituted meanwhile as the Laboratory of Genetics. Previous to this, Vavilov had held the post of Director of the All-Union Institute of Plant Industry, which had been refounded from the older Bureau of Applied Botany. After

[1] Наследственность есть как бы концентрат условий внешней среды, ассимилированных растительными организмами в ряде предшествующих поколений.

Filipčenko's death he held both posts until 1940, when he was replaced in the former by Lysenko and in the latter by Eĭhfeljd (Eichfeld).

Vavilov's fame in the genetical world was due largely to the collecting expeditions, which he organized into many parts of the world, in order to assemble genetical material for the breeding of improved crop plants. He had developed a theory of Centres of Origin, having some points in common with Willis's Age and Area concept, postulating that genetical diversity was greatest at the loci at which particular circles of forms had originated. The various parallelisms that he observed between the modes of variation exhibited by closely allied species and subspecies led him to formulate his Law of Homologous Variation.

Throughout his career, Vavilov was a staunch protagonist of Mendelian theory, and when, after 1932, the Russian genetical controversies began, he was their principal defender against the attacks of Lysenko's school. The further history of these controversies will be treated in detail later.

Other early exponents of Mendelism in Russia include Karpečenko, Lepin, Sapehin, Delaunay, Navašin, Dubinin and Serebrovskiĭ. Of these, Karpečenko, who made pioneer investigations into intergeneric hybrids in the Cruciferae, Lepin, who wrote on the genetics of wheat, and Navašin the eminent cytologist, played only minor roles in the public discussions, although both Karpečenko and Lepin criticized Lysenko's theories when they were first introduced. Navašin continues to publish work occasionally, while Karpečenko, who worked with Vavilov at the Institute of Plant Industry, died during the war.

Sapehin, Lysenko's predecessor at the Odessa Research Station, and Delaunay, a former director of the Maslov Research Station, both criticized Lysenko's theories in the year 1936, when the genetical controversy was well under way. Both were too old to play much part in the further developments that followed.

Dubinin, who formerly worked at the Timirjazev Research Institute of the Communist Academy and is now at the Institute of Cytology of the Academy of Sciences, has done some distinguished work on the population genetics of *Drosophila*. He took a prominent part in the fourth session of the Lenin Academy of Agricultural Sciences, where he defended Mendelian genetics against Lysenko and Prezent. He was heavily criticized for so doing and has since refrained from venturing further into controversial issues. Serebrovskiĭ, head of the Laboratory of Genetics at Moscow University, has made many contributions to animal genetics. He has been one of the most consistent and fearless critics of Lysenko, and in spite of strong personal attacks, has refused to make any substantial concessions to Lysenko's school.

Two of the most eminent Russian geneticists had already left the U.S.S.R. Dobzhansky, a former research worker at the Laboratory of Genetics of the Academy of Sciences, is now in the U.S.A. and has become one of the leading exponents in the whole world of neo-Darwinism. Timofeev-Ressovskiĭ, who began his researches at the Laboratory of Experimental Biology at Moscow, made pioneer investigations into the effects of X-rays on nuclear phenomena, and has done most of his work in recent years in Germany.

Foreign geneticists have also played a conspicuous role in the development of Mendelism in Russia. Bridges, the American authority on *Drosophila* genetics, worked at the Laboratory of Genetics of the Academy of Sciences for a few years, while Muller, who did so much to develop the theory of X-ray effects on chromosomes, was one of the principal antagonists of Lysenko at the 1936 conference. Kostov, the Bulgarian geneticist, also worked at the Laboratory of Genetics of the Academy of Sciences for some years, and he associated himself with Konstantinov in 1936 in a critical analysis of Lysenko's theories.

One other somewhat half-hearted Mendelian geneticist has played a prominent part in the genetical controversies, and that is Meĭster, the former head of the Saratov Agricultural Experimental Station (later renamed the Institute of Grain Industry). Until 1935, his views appeared to have differed little from other Mendelian geneticists, but in the following year, when he acted as chairman at the genetics conference of 1936, he attempted to compromise between Mendelism on the one hand, and Lysenko's views on the other. He deprecated Muller's remarks on the stability of the gene, emphasized the theoretical necessity of adhering to Soviet Darwinism, was mildly appreciative of Mičurin's work, while at the same time dissociating himself from the extreme anti-Mendelian views of Lysenko, Prezent and Perov. After this meeting, he took comparatively little part in the controversies that raged, and has since died.

Dialectical Biology

So accustomed have biologists in general and geneticists in particular become to theorizing without explicit reference to philosophy, or to theorizing using the philosophies of realism or

materialism as a basis, that the replacement in Russia of these philosophies by dialectical materialism tended not to make itself felt until after a considerable lag. In the Soviet Union the philosophy of dialectical materialism as developed by Marx, Engels and Lenin, was made official after the October Revolution, yet it had practically no visible impact on Russian Mendelism for a period of ten years or more. It is true that such geneticists as Vavilov and Dubinin declared their acceptance of dialectical materialism, but there is but little evidence that this philosophy exerted any influence on their genetical theories.

Yet, while those actively engaged in scientific work had little practical use for dialectical materialism, there were several exponents of the philosophy who tried to show that dialectics had an important application in this field. Such an attempt was in fact made by Engels himself though it made no impression in the scientific world until quite recently. Few of these earlier efforts were in any way remarkable, since their exponents were seldom sufficiently equipped with critical scientific knowledge.

In Russia, although dialectics were ignored at first, a new generation has arisen accustomed to thinking along dialectical lines, and has made an attempt to base science on the new philosophy. Of these, Krenke, who worked on plant physiology at the Timirjazev Research Institute of the Communist Academy, was one of the most distinguished. His theory of Cyclic Senescence and Rejuvenation was an attempt to construct a theory of vegetative development on the basis of dialectical materialism. Although obscure in places, his main assumptions have a certain degree of internal coherence when viewed in relation to dialectical materialism. He views vegetative growth as arising from the struggle between two dialectical opposites, identified by Krenke as the processes of senescence and rejuvenation, from whose interaction the development of the plant proceeds. This theory he applied to many problems of the physiology of development, also to various points in the theoretical systems of Mičurin and Lysenko.

Another keen exponent of dialectical materialism is Žebrak,[a,b,c] who has laboured consistently to try and show that most of the tenets of Mendelian genetics are in accordance with Marxian philosophy. In 1935 and the following year he wrote against Weismann's theory of the germplasm, which he stigmatized as dualistic, i.e. attributing to organisms, not a unity in opposites as dialectical materialism would hold, but the co-existence of two quasi-independent and changeless components, the germplasm and the soma. At the same time, he defended genetics against Lysenko's attacks, pointing out that Lysenko's own conclusion that F_2 hybrids were incapable of transgressive segregation contradicted the facts. He also quoted experiments in which the genetical interaction between scion and stock claimed to occur by Lysenko could not be detected, and he pointed out that Lysenko's theories were Lamarckian in their implication. An important paper by Žebrak entitled "Genetics and the Theory of Phasic Development of Plants" appears never to have been published. It contains a strongly critical review of Lysenko's theories and drew forth a reply from Prezent[b] in 1936, from whose quotations it is possible to form some idea of its contents.

In 1937, Žebrak[d] criticized Vavilov's theory of Centres of Origin and his Law of Homologous Variation, antithesizing these to the views of Darwin. Muller's views on the stability of the gene were also subjected to unfavourable criticism.

At the 1939 genetical conference, Žebrak defended Mendelism once more, pointing out that the plant breeding achievements of the U.S.S.R. were based upon it. He re-emphasized the concordance between Mendelism and Darwinism, illustrating his argument with quotations from Timirjazev and Mičurin.

More recently still, in the American journal *Science*, Žebrak[e] has characterized Lysenko's views as naive and speculative, and has accused him of applying dialectics unsoundly, a recent and suggestive development of the genetical controversy which will be considered later.

Prezent, one of the most influential Soviet biologists supporting Lysenko, is perhaps the principal exponent of dialectical genetics, and has apparently exerted a most profound influence on the development of Lysenko's genetical theories. There is indeed evidence that the full elaboration of the genetical system of Lysenko is principally due to Prezent, who entered into close association with Lysenko after the latter moved to Odessa. The application of dialectical materialism and the revival of the so-called Soviet Darwinism only appears to characterize Lysenko's theories after his collaboration with Prezent. It will be convenient, however, to postpone a detailed consideration of this point until the next section.

Lysenko

The Ukrainian biologist Lysenko was born in 1898 of a peasant family living in the province of Poltava. He entered the Horticultural Institute of Poltava in 1913, passing thence to the

Umansk Institute of Horticulture where he remained until 1921. After acquiring experience as a plant breeder at Kiev, he was appointed to the newly founded research station of Gandža in Azerbaidjan, where he began his experiments on vegetative period. From these investigations he arrived at his well-known theory of phasic development, which, together with the practical technique of vernalization, brought Lysenko a world-wide fame.

In 1929, Lysenko expounded his principles to the genetics congress held that year in Leningrad, and in the next year he was transferred to Odessa, where he soon succeeded Sapehin. Here he continued his researches on vernalization and plant breeding, publishing in 1934 a very moderate presentation of his theories regarding the mode of interaction between environmental factors and genetical constitution. It is true that he charges "formal" geneticists with too rigid a belief in the absolute invariance of genetical characters, but in general, his treatment is characterized by balance and carefulness.

It was not until the following year, 1935, that he set out to overthrow classical genetics and to erect in its place his own system. This project, which gathered momentum with each successive year, may be dated from the publication of the book entitled *Plant Breeding and the Theory of Phasic Development*, which Lysenko and Prezent brought out together that year.

In this account many of the characteristic tenets of Lysenko's genetical system are explicitly mentioned for the first time. The importance of phasic conditions in determining such plant characteristics as flowering period, yield, quality and frost resistance is emphasized. It is also stated that, as a corollary to the theory of phasic development, it should be possible to predict the dominance or recessiveness of several important economic characteristics in F_1 intervarietal crosses. This prediction is made by assuming that the development of such characters is controlled principally by the action of limiting factors[1] which may operate at either one or both of the two phases then recognized by the theory of phasic development. If then a cross is made between two varieties carrying different limiting factors, it is further assumed that the operation of the limiting factors derived from each parent will be neutralized by the corresponding factors encouraging normal development in the opposite variety. Thus, if one parent in an intervarietal cross carries a limiting factor a retarding development in the first phase and yet develops normally in the second phase through the operation of a growth factor B, and if the second parent develops normally in the first phase through the operation of a factor A, yet is retarded in the second phase by a limiting factor b, then it is concluded that the F_1 hybrid will develop more readily than either parent, since the two limiting factors a and b would be neutralized in their effect by A and B. These factors are not regarded as Mendelian factors, since it is assumed that a limiting factor is always neutralized in the presence of the normal, or to express the matter in genetic terminology, is necessarily recessive. Both earliness and vegetative vigour were regarded by Lysenko and Prezent as determined by such a limiting factor interaction, and consequently they drew the conclusion that all F_1 hybrids were as early as the earlier parent or earlier, and as vigorous as the more vigorous parent or more vigorous. They insisted that, when the necessary environmental conditions were provided, no F_1 hybrid could be later than the earlier parent or less vigorous than the more vigorous parent.

Developing this line of thought further, Lysenko and Prezent maintained that, since in general a hybrid would combine within itself the potential adaptabilities to the environments of both of its parents, it would be adaptable to a wider range of environmental conditions than either parent alone. Furthermore, since F_2 segregation was regarded as inevitable in their earlier writings, it was considered that the F_2 and subsequent generations would be less adaptable than the F_1, for the adaptabilities combined in the F_1 would become separated by subsequent segregation.

Because of these considerations, Lysenko and Prezent claimed that no character such as earliness or vegetative vigour could be more strongly expressed in the F_2 or later generations than in the F_1, i.e. that transgressive segregation for earliness or vigour was impossible. But they insist again that the plants studied must be grown under appropriate phasic conditions.

Regarding the general theory of dominance, Lysenko and Prezent adopted Mičurin's theory that the characters best adapted to the local environment are dominant in hybridization, and that, under different environments, dominance may be reversed. The limiting factors mentioned above may, according to this viewpoint, be only limiting under certain environmental conditions. They may be favourable factors elsewhere, in which case they would be dominant instead of recessive in an F_1 hybrid. The capacity of responding adaptively to different environments in the way indicated was regarded as having originated through a long process of natural selection.

---

[1] узкие места, literally "narrow places".

Such theories naturally provoked sharp criticism amongst Russian Mendelian geneticists and controversy flared up immediately. Lysenko and Prezent met objections with considerable skill and began publication in the same year of the journal *Jarovizacija*, through which their views were disseminated. This journal continued under the joint editorship of Lysenko and Prezent for a few years, coming out later under the sole editorship of Lysenko. It is one of the most important sources for the history and theory of Lysenko's system.

In the first number of this journal, Lysenko[b] develops his ideas considerably further, attacking the genetical notion of the pure line as inconsistent with the facts. Reviving Darwin's theory that small differences in environmental conditions are able to modify hereditary constitution, he sets out to show that pure lines are not uniform, but consist of a number of physiological variants, each having arisen in response to minute environmental differences. Self-fertilization he regards, following Darwin and Timirjazev, as invariably disadvantageous, the explanation offered being similar to that given above in respect of intervarietal crossing. The process of selfing is believed to result in a segregation of adaptability, so that in each generation the pure line segregates in respect of the physiological factors believed to differentiate it into different strains.

To counter this supposed deterioration, Lysenko proposes that the pure line be rejuvenated by emasculating a few individuals and artificially pollinating these with a pollen mixture derived from other individuals of the same pure line. It is believed then that the recombination of adaptabilities originally combined in an ancestral form and later dispersed through segregation following selfing, will help to restore the vigour of the pure line. Lysenko also believes that the ovum is able to select that pollen grain from a mixture which has the capacity to produce the best adapted offspring, this process being known as selective fertilization. It seems likely that this latter theory was derived in part from Darwin's notion of pollen prepotency, in which competition between pollen grains was postulated. According to Lysenko[b] and Prezent, the capacity of the ovum to select advantageous pollen grains is a result of natural selection.

So far, mention has only been made of the positive side of Lysenko's theories, but it is important to realize that these are intimately bound up with a spirited attack on genetical science as commonly accepted. This attack, which began somewhat tentatively, gathered strength rapidly, and extended from a criticism of isolated details to a denunciation of the whole conceptual basis of Mendelian genetics.

The details of the controversies that form such a conspicuous element in the history of Soviet genetics will be given in the next section. The remainder of this section will be devoted to giving a brief outline of the later development of Lysenko's ideas.

In 1936, the year of the second important genetical congress, Lysenko[h,j] reiterated his previous theories, and replied to Vavilov's claim that flax exhibited transgressive segregation for earliness, by suggesting, either that Vavilov's report was untrue, or that his method of calculating earliness was unsatisfactory. He also denounced the chromosome theory of heredity, denying the importance either of chromosomes or genes in determining hereditary behaviour. Examples were quoted of increased yields following intravarietal hybridization, and the theory already mentioned of the effect of the environment on hereditary constitution was extended to cover experiments in which it was claimed that the winter wheat Kooperatorka had been converted into a spring wheat by a suitable vernalization technique.

The Mendelian laws of heredity were next subjected to attack, many of the arguments used being derived from Timirjazev. Instances of F_2 segregation not conforming to a 3 : 1 ratio were quoted as disproving Mendelism, also the existence of non-segregating F_2 progenies, a type of inheritance that Timirjazev had termed Millardetism after the researches of Millardet in France at the end of the nineteenth century.

At the 1939 genetical congress, Lysenko maintained his theory of the possibility of directional modification of the genotype and reiterated his attacks on Mendelism. He accused Vavilov of inconsistency in admitting the importance of environmental conditions in genetical studies and declared that geneticists were endeavouring to repress the new developments ushered in by himself. The charge was also made that genetics was closely associated with the racial theories current in fascist Germany.

In the following year, Lysenko[p] attacked the whole notion of applying mathematics in biology, a line of thought, previously developed by Prezent[b] in *Jarovizacija*. He urged that Mendelism should be excluded from university curricula and should be replaced by his own system. Reinforcement of his own position was sought from experiments on the interaction of stock and scion in grafting. He claimed that hereditary changes could be induced by grafting, and quoted in his support the writings of Darwin, Mičurin, Timirjazev and Burbank.

The beginnings of Lysenko's[r] nutrient theory of heredity are also discernible in his publications in this year. This theory, which appears to be derived principally from the speculations of Darwin, Mičurin, and Burbank, is to the effect that the genotype is the resultant of nutrients[1] absorbed from the environment by the organism and converted into the substance of the organism by the process of "assimilation".[2] Under optimum conditions it is believed that the organism remains constant and absorbs nutrients to which it is adapted, thereby suffering no change in its hereditary constitution. Under adverse conditions, however, Lysenko suggests that the hereditary constitution of the organism becomes modified; it is believed to absorb nutrients from the environment of a type dissimilar to the usual, and to combine with these to form a new compound, which then becomes the new hereditary substance of the organism. By maintaining the modified plant under constant conditions it is believed to become stabilized and to regain its former "conservatism".

This theory of the genotype as assimilated environment was developed during the three following years and was expounded at length in 1943 in an article entitled "On Heredity and its Variability". It will be considered in detail later.

The rapid dissemination of Lysenko's theories in the Soviet Union appears to have been caused by the coincidence of several factors, amongst which official support is to be included. M. M. Zavodovskiĭ points out in addition that Lysenko has made great use of the popular press in disseminating his views, whence his popularity with the workers on the state and collective farms. Lysenko himself was offered many important posts; he became President of the Lenin Academy of Agricultural Sciences in 1938 and replaced Vavilov about 1940 as Director of the Institute of Genetics of the Academy of Sciences, of which academy he is a member. Twice he was awarded the First Stalin Prize, and he also received the Order of Lenin. He holds the office of Vice-Chairman of the Supreme Soviet, although he does not appear to have been an active member of the Communist Party until comparatively lately.

These signs of official recognition have doubtless played some part in strengthening his position in the various controversies, now to be treated.

Controversy

The various issues underlying the recent history of genetics in the Soviet Union have been clarified to a certain extent by the genetical congresses that have been held to elucidate them. Some of the remarks made at these meetings may not of course represent the final opinion of their originators, and a certain latitude must be allowed for statements made in the heat of discussion. But when this is done it is possible to determine fairly exactly the points around which controversy has raged.

The first important congress requiring mention is the All-Union Conference on the Planning of Genetics and Selection, which was held at Leningrad in 1932. At this meeting the note of urgency was sounded, which Lysenko turned to his own profit a few years later. The need of improving varieties and raising the standard of seed production was emphasized, a subject that had provoked Stalin to demand from plant breeders a significant improvement within a period of four to five years. Practical results were laid down as the first desideratum of Russian geneticists. A resolution was also passed that genetics and plant breeding were to conform with dialectical materialism, a point which proved to Lysenko's advantage later.

Lysenko and Prezent, as already described, introduced their new genetical theories in 1935, and this precipitated a heated controversy which culminated in the genetical congress held at the fourth session of the Lenin Academy of Agricultural Sciences in 1936.

This conference is extremely important historically since a very large number of Russian geneticists took part and expressed views for or against the genetical system of Lysenko. But there are several difficulties involved in determining what exactly occurred. There are three main sources of information, the reports in *Selekcija i Semenovodstvo* and *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva*, and the articles in *Jarovizacija*. Significant differences in the various reports in respect of the same address are noticeable, mainly omissions of various points. It will be found that many of Lysenko's attacks on Mendelian geneticists, recorded in *Jarovizacija*, are omitted in the other two journals. Prezent's remarks are only to be found in *Jarovizacija*, although criticisms of his point of view are reported in *Selekcija i Semenovodstvo*. The final addresses of Muralov and Meĭster are reported also in the *Bjulletenj Vsesojuznoĭ Akademii S.-H. Nauk im. V. I. Lenina*. The account given below has been derived from all these sources and does, it is hoped, give a fairly faithful picture of the actual course of events.

---

[1] пища. [2] ассимилирование.

The chair at this meeting was taken by Meĭster,[b] who tried ineffectually to compromise between the opposing parties. He supported Lysenko's attack on the theory of pure lines but dissociated himself from the claim that selfing inevitably leads to deterioration; he was cautiously disapproving of Lysenko's claim that winter wheat could be converted to spring wheat by vernalization and deprecated Lysenko's outspoken attack on the chromosome theory of inheritance. On the other hand, Meĭster reproved Muller and Serebrovskiĭ for maintaining the stability of the gene, and declared his respect for the plant breeding theories of Mičurin.

Serebrovskiĭ defended Mendelian genetics with vigour to the grave displeasure of some of the congress, and accused Lysenko of resurrecting Lamarckism. He was supported by Dubinin, who however appeared nervous and was much concerned to vindicate genetics from the charges of formalism and anti-materialism. Vavilov[b] expounded Mendelian theory as best he could but was unfavourably received and heavily criticized afterwards for alleged inefficient direction of the Institute of Plant Industry, and for having made no adequate reply to Lysenko's claims.

Muller made a powerful defence of Mendelism and suggested that its traducers were repeating the tactics of Mach, though masquerading as defenders of dialectical materialism. He assembled the cyto-genetic evidence supporting modern genetical theory and accused Lysenko of Lamarckism. Other speakers supporting modern genetics included Levitskiĭ, Šehurdin, Lisicyn, Konstantinov, Kislovskiĭ, Navašin, who urged the replacement of controversy by experiment, Semevskiĭ, Koljcov, Sapegin and Karpečenko.

The most successful disputants were undoubtedly Lysenko[j] and Prezent,[e] whose frequent references to practical issues and whose attacks on Mendelian theory as inconsistent with dialectical materialism and Darwinism appear to have made a great impression on the non-scientific members of the congress. Lysenko was seconded by B. M. Zavodovsikiĭ, Jakovlev, Šlykov, Nurinov, Borisenko, Bosse, Dolgušin, Utehin, Cicin, Perov, Oljšanskiĭ and Pleseckiĭ, and also received support from Ermakov, Zarkevin, Sjutkin and Grebenj, followers of the successful animal breeder Ivanov. The criticisms of these investigators were along similar lines, accusing Mendelism of idealism, mechanism, anti-Darwinism, and lack of practical usefulness.

Sinskaja, Pisarev and Pokrovskiĭ were cautious and non-committal in their utterances, while Krenke, in a benign address, pointed out that there was much to be said both for Mendelism and for Lysenko's theories. Short abstracts of the speeches in the general discussion were published in *Selekcija i Semenovodstvo* (cf. Anon.[a]).

Vavilov's position at this time had undoubtedly become insecure. It is difficult to disentangle the various underlying reasons, but it seems that a definite manoeuvre was set afoot to bring about his dismissal from the Institute of Plant Industry. An extremely virulent attack upon him was published by Kolj in 1936, in which he was accused of having failed in his duty of applying genetics to the practical problem of crop improvement, of having sent worthless expeditions to collect material for the World Collection instead of concentrating on local varieties, of being more interested in formal genetics than practical application, of showing a suspicious friendliness to genetical ideas emanating from fascist Germany, and of being unsympathetic to the theories of Mičurin and Lysenko. Vavilov's work was defended in reply by Žukovskiĭ, who wrote an appreciation of the World Collection and its utility, but this did little to turn the tide in his favour. After the genetical congress mentioned above his position was so uncertain that a report appeared in the *New York Times* of his arrest, together with the geneticist Algol. Algol's arrest on the charge of Trotskyite activities was confirmed, but the report of Vavilov's arrest proved to be unfounded and was retracted by the *New York Times*, who published at the same time a telegraph from Vavilov vindicating freedom of scientific research in the Soviet Union.

In 1937, Žebrak[d] criticized Vavilov's theory of Centres of Origin and the Law of Homologous Variation, and criticism of Vavilov on these and other grounds increased until the third major genetical congress which was entitled "Under the Banner of Darwinism," and was held in the office of the Commissariat of Agriculture of the U.S.S.R. at Moscow. It was reported by Dmitriev in *Jarovizacija*.

Once again, the necessity of producing practical results from genetical theories was emphasized, also the necessity of basing genetics on dialectical materialism. Lysenko re-affirmed his belief in the possibility of changing the hereditary constitution of plants in any desired direction and re-assembled his arguments against Mendelism. He was supported by Frenkel, Eĭhfeljd, Jurjev, Puhaljskiĭ and Lukjjanenko, who declared their antipathy to formal genetics and affirmed their faith in intravarietal crossing and the plasticity of the genotype.

Vavilov, in his reply to his critics, defended once again the validity of Mendelian genetics, and declared that, in spite of contrary statements, the achievements of Russian plant breeders

were in fact based upon it. He asserted that the distinction between phenotype and genotype was legitimate, that Mendelism was the best guide to inheritance studies, that the chromosome theory of inheritance was too well founded to be shaken, and that Johanssen's theory of the pure line, as understood by modern geneticists, was a helpful concept. He admitted also the truth in Lysenko's contention that plants should be bred under optimum conditions, though he went on to criticize Lysenko's ignorance of genetical literature and charged him with scheming to repress the teaching of genetics.

Žebrak appeared at this congress in his familiar role of peace-maker between genetics and dialectical materialism. He endeavoured to show that Mendelism was consistent with Soviet Darwinism by using the technique of its adversaries, quoting relevant passages from Darwin, Timirjazev and Mičurin.

A comprehensive reply to the defenders of Mendelian genetics was furnished by Prezent, who maintained that the results of intravarietal crossing disproved the pure line theory. He accused Vavilov of inconsistency in admitting the usefulness of optimum conditions in breeding, and replied to Žebrak by quoting passages of Mičurin and Timirjazev, minimizing the importance of Mendel's work. Instances of F_2 segregations not in accordance with a 3 : 1 ratio were again quoted as evidence against Mendelism, and in response to the charge of ignoring world science, he replied that, as regards American genetics at least, Burbank was a better guide than Morgan.

Lysenko, in a second speech, also accused Vavilov of inconsistency in admitting the utility of breeding under optimum conditions, and countered the charge of repression by the assertion that geneticists were attempting to suppress his own theories. He repeated the charge that genetical theories logically lead on to the racial theories of German fascism.

At this conference again, Lysenko's party was victorious and his triumph was reaffirmed by the speeches of Lysenko, Keller, Cicin and Prezent at a meeting of the Lenin Academy of Sciences, reported by Halifman, and held in the same year. Vavilov's position became even less favourable. In 1940, Dvorjankin published another criticism of the theory of Centres of Origin, which he accused of causing neglect of local races. In the same year, Vavilov was finally relieved from his posts as Director of the Institute of Plant Industry and as Director of the Genetics Institute of the Academy of Sciences, to be succeeded in the latter by Lysenko. He died a few years later, possibly in 1942.

Genetical Institutes.

In most countries in Western Europe and America, the development of genetical science has taken place in the universities, and, owing to the international connexions between these bodies, has developed as a homogeneous whole. Such has not been the case in Russia, where the October Revolution dealt a blow to the universities from which they have hardly yet recovered. The loss of scientific personnel at the time of the revolution, and the refusal of many of the older scientists to co-operate with the new regime caused a diversion of biological research from the universities to the numerous technical institutes which had already been founded, and which grew up rapidly after the revolution, a switch over which has been discussed already by Maximov.

Today, according to Huxley, the only regular course in Mendelian genetics at a Russian University is that provided by Moscow University, where genetical research is still being maintained by Serebrovskiĭ and Dubinin. At Leningrad, Lysenko's theories have been officially accepted, and are being developed there by Prezent, who moved from Moscow to Leningrad some years ago. As far as is known, Mendelian genetics receives no official recognition today in this university.

The default of genetical research in the universities has been compensated to some extent by the research encouraged in the laboratories of the various biological and agricultural institutes. In this connexion the institutes of the Academy of Sciences have been extremely important. The Laboratory of Genetics was founded as the Bureau of Genetics in 1921 and was re-organized in 1930. Its three directors, Filipčencko, Vavilov and Lysenko have all played conspicuous roles in the history of Russian genetics. The All-Union Institute of Plant Industry was re-founded after the October Revolution on the basis of the older Bureau of Applied Botany. It was directed by Vavilov until 1940, and was one of the principal centres of Mendelian genetical research in the Soviet Union until quite recently, and included amongst its research schemes the sending of collecting expeditions into all parts of the world and the maintenance of the famous World Collection of economic plant varieties. A more recent foundation that has been actively concerned in the genetical controversies of the U.S.S.R. is the Lenin Academy of Agricultural Sciences, which is at present under the direction of Lysenko.

Other institutes of lesser importance include the Timirjazev Research Institute of the Communist Academy at Moscow, the Timirjazev Agricultural Academy, originally the Agricultural Academy of Petrovskoe Razumovskoe, also at Moscow, and the regional research stations of Odessa and Saratov.

It is possible that some of the characteristic traits of the recent genetical controversies in the Soviet Union may be attributable to the fact that the universities have played so insignificant a role in genetical research. The ignorance of world science displayed by several of the protagonists and the frequent use of alogical[1] discourse in academic debate are features seldom tolerated in university circles. It is therefore necessary when trying to discover the reason why such forms of argument appear so frequently in the Russian literature, to bear in mind these circumstances.

Publications

Frequent allusion has already been made to the extreme difficulty that besets anyone endeavouring to arrive at an impartial estimate of the history and present state of genetics in the Soviet Union. This difficulty is nowhere more acute than in attempting to interpret the published literature, which is the principal source of knowledge on these topics.

It would appear that editorial selection is extremely active in the Soviet Union, and consequently the frequency of articles cannot be used as evidence of the prevalence of any point of view, at least not without supplementary evidence. Moreover, it is by no means certain that authors invariably express their own opinions, especially in respect of such controversial subjects as the value of dialectical materialism in scientific work. It is noteworthy that, whereas some authors permeate their articles with the language of dialectics, others merely refer to dialectical materialism in an apparently perfunctory introductory or concluding section. Such differences in style may of course be purely fortuitous, but they cannot be ignored in trying to ascertain the real views of the authors concerned.

In the matter of Lysenko's genetical theories, the attitude of editors seems to have been very significant. It has been noteworthy that, at first, only a few journals published Lysenko's articles and these were semi-official agricultural publications such as *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*), renamed later *Socialističeskoe Seljskoe Hozjaĭstvo* (*Socialist Agriculture*), and *Semenovodstvo* (*Seed Growing*), renamed later *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*), and of course *Jarovizacija* (*Vernalization*), the journal edited by Lysenko and Prezent. In the two agricultural journals mentioned, Lysenko's notions first received prominence in 1935, during which year and the following year they became the subject of a spirited controversy, and drew rebuttals from such authors as Sapehin, Delaunay, Konstantinov *et al.*, Žebrak,[b] Meĭster[a] and Vavilov.[a] After that time Lysenko's position as far as these journals were concerned seems to have become unquestioned, and critical articles directed against his theories cease to appear. In *Jarovizacija*, only articles by the supporters of Lysenko were usually accepted.

A similar state of affairs obtained in the case of the horticultural journals. Practically all of Mičurin's articles were published in semi-popular horticultural periodicals and not in publications devoted to pure biology; later, the journal *Za Mičurinskoe Plodovodsto* (*Horticulture by Mičurin's Methods*) was devoted exclusively to propagating Mičurin's ideas. The horticultural journals were among the earlier supporters of Lysenko, and papers by himself or his followers became frequent after 1938 in such journals as *Ovoščevodstvo* (*Vegetable Growing*), *Plodoovoščnoe Hozaĭstvo* (*Fruit and Vegetable Growing*), renamed later *Sadovodstvo* (*Horticulture*) and then *Sady i Ogorody* (*Fruit and Vegetable Gardens*), and *Ovoščevodstvo i Kartofelj* (*Vegetables and Potatoes*). The forestry journal *Lesnoe Hozjaĭstvo* (*Forestry*) began publishing papers by Lysenko's adherents in 1939.

The journals devoted to pure science appear to have shown considerable reluctance in accepting articles supporting Lysenko's theories, and he receives little space in either *Priroda* (*Nature*), the *Bulletin de l'Académie des Sciences de l'U.R.S.S.* or *Vestnik Gibridizacii* (*Hybridization*). The *Bjulletenj Vsesojuznoĭ Akademii S.-H. Nauk im. V.I. Lenina* (*Bulletin of the Lenin Academy of Agricultural Sciences*) appears to have swung over to Lysenko's standpoint since he became President of the Lenin Academy, and a similar shift may be detected in *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*). The *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.*, which is perhaps the scientific publication of the Soviet Union best known in other countries, has published little by Lysenko[p] beyond the controversy with Kolmogorov in 1940. Similarly,

---

[1] See Chap. III.

Sovetskaja Botanika (*Soviet Botany*) lent little support to Lysenko until 1941, and has since shown little enthusiasm in publishing his theories.

Other journals that have played a role in disseminating Lysenko's idea are *Len i Konoplja* (*Flax and Hemp*) and the *Bjulletenj Instituta Zernovogo Hozjaĭstva Jugo-Vostoka S.S.S.R.* (*Bulletin of the Institute of Grain Husbandry, South-east U.S.S.R.*) *Saratov*, the latter, however, tending to emphasize the contributions of Mičurin rather than Lysenko.

It is, of course, possible again that the late acceptance by journals devoted to pure science of Lysenko's theories is merely fortuitous but the fact can hardly be ignored. There is also evidence that a reaction is setting in against the ready acceptance of Lysenko's theories, and several journals which would have ignored articles hostile to Lysenko's genetical system in 1940 now seem willing to consider both sides of the matter. This question, however, dealing as it does with the contemporary situation, is still obscure.

The Situation Today

No effort will be made to give any but the most tentative outline of the present position of Russian plant genetics, since the information to hand is not adequate to the formulation of any more than conjectures. It has already been mentioned that there is considerable uncertainty as to how far published articles do in fact represent their author's true opinions. The gradual disappearance from the published literature of criticism of Lysenko's theories cannot be taken as a necessary indication that Lysenko's theories have in fact won universal support. In this connexion it is interesting to discover that the geneticists Harečko-Savickaja and Bolsunov, who continued their researches in the Ukraine under the German occupation, published critical articles on Lysenko's theory of selective fertilization in the *Zeitschrift für Pflanzenzüchtung*. More recently still, in the American journal *Science*, Žebrak[e] has stigmatized Lysenko's theories as naive and speculative, and has condemned his application of dialectical materialism as unsound. He insisted, moreover, that Lysenko had received official recognition on account of his agronomic studies alone, and that the Soviet authorities had not officially endorsed his genetical theories.

Žebrak has, however, rather underestimated the official support extended by the Soviet authorities to Lysenko in the past. At the 1939 genetical conference, Benediktov, the People's Commissar of Agriculture, is reported by Dmitriev to have stated that "the People's Commissariat of Agriculture of the U.S.S.R. supports Lysenko in his practical work and in his theoretical views and recommends the breeding stations to apply his methods in seed production and breeding work".[1] It is possible now, of course, that such support is no longer being extended and there is no doubt that there is a considerable undercurrent of opposition to his theories on the part of many Russian geneticists today. Although Vavilov and Karpečenko are no longer alive, Serebrovskiĭ, Dubinin, Navašin and Žebrak are still actively working along Mendelian lines, even although they maintain a certain reserve on controversial topics. Some sort of recognition of the value of dialectical materialism in scientific work and a tendency to exalt the work of Darwin, Mičurin and Timirjazev appears to characterize almost all Russian genetical publications, irrespective of the attitude taken to Lysenko's theories, but as already pointed out, it cannot be assumed without further evidence that such convictions are in fact held by all authors.

Lysenko is undoubtedly one of the best-known figures in Russia today and his prestige among collective and state farm workers appears to be immense. He is also Vice-Chairman of the Supreme Soviet, President of the Lenin Academy of Agricultural Sciences, and Director of the Institute of Genetics of the Academy of Sciences, as well as holding several other important posts. His colleague Prezent holds a chair in the university of Leningrad, in which body his ideas appear to have received official sanction. It is not certain that Lysenko is as highly regarded by his fellow geneticists as by the farm workers, and it is possible that opposition to his theories may flare up again given suitable circumstances. The most that can be concluded about the present situation is that it is remarkably unstable and may develop rapidly in the near future.

---

[1] Наркомзем СССР поддерживает академика Лысенко в его практической работе и в его теоретических взглядах и рекомендует селекционным станциям применять его методы в семеноводческой и селекционной работе.

III. PSYCHOLOGY

To those accustomed to the convention of impersonality generally observed by western scientific writers, any discussion of psychological questions relating to the mental attitudes of research scientists may seem irrelevant. Such a convention, however, is not observed in the Soviet Union, and scientific discussions there often turn about the motives of the protagonists of rival theories, rather than around questions of intrinsic reasonableness. It is indeed possible to analyse the various genetical theories put forward in Russia, setting aside such questions of psychology, and this procedure will be followed in the remaining chapters. But it is not possible to understand how these theories came to be put forward, or why they are so ardently championed, unless the underlying psychological motives are ascertained. Furthermore, the types of scientific discourse followed in Russian journals are frequently alogical[1] in form, that is, do not concern themselves so much with stringent argumentation from factual data, but analyse instead the psychological issues involved in accepting or rejecting the scientific concepts under consideration. For this reason, attention must be paid to this type of procedure if a comprehensible account is to be rendered of the Russian genetical theories.

In Western Europe, the convention of impersonality derives from the logicians of the Middle Ages from whom it was inherited by the seventeenth century scientists. It was commonly believed that the concepts derived by legitimate inference from scientific observation were unconditionally true, granted that the observations were accurate and the inference valid. So convinced were scientists of the seventeenth and eighteenth centuries of the absolute nature of their conclusions that Kant used this confidence as a prime consideration in his *Critique of Pure Reason.*

Marx, in the nineteenth century, attacked this complacency. By insisting on the primacy of matter and the secondary importance of mind, he reinterpreted the history of ideas as a reflexion of the concurrent history of material conditions, rather than as an independent progressive illumination of the human intellect. In his writings, and in those of Engels,[a] the procedure is often adopted of analysing ideas, not in respect of their intrinsic reasonableness, but by regarding them as inevitable consequences of material conditions, the latter interpreted along Marxian principles. This shift of emphasis from logical to historical analysis results in a psychological treatment in which the motives of scientists, conditioned by their material environment, are treated in preference to a detailed examination of the content of their ideas. This type of procedure appears to have found a congenial soil in Russia. Pre-occupation with motivation is characteristic of the greater Russian novelists such as Dostoevsky and Tolstoy; it also characterizes the early Russian Marxist Plehanov and, to an extraordinary degree, his successor Lenin.

Throughout the whole length of his classical work *Materialism and Empiriocriticism*, Lenin inveighs against the philosophical views of many late nineteenth century thinkers, not by the traditional method of logical refutation, but by a form of associative discourse, in which he shows that the views that he is denouncing either disagree with those of Marx or Engels, or conform to some extent with those of Berkeley, Hume or Kant. The correctness of the views of Marx and Engels are largely assumed, also the falsity of the idealist and sceptical trends in philosophy.

It must not be supposed that Russian geneticists are antipathetic to logical reasoning, for both Lysenko and Prezent have written many long passages of stringently argued discourse, well-nigh impeccable from a logical standpoint. What is characteristic is not an absence of logic, but an intermixture of logical and alogical methods of procedure. It frequently happens that two or three pages of careful argumentation are followed abruptly by a sudden dart into speculation of an extraordinary type, or into a minute analysis of the motives of their critics, or into quotations from some approved authority. Such sudden metamorphoses in the style of discourse will not be unfamiliar to those conversant with Russian literature. It is also important to bear in mind that alogical discourse is not necessarily illogical. It is quite possible to derive valid conclusions by invalid argumentation, but it is not possible to demonstrate that the conclusions so reached are in fact true. In arguing by authority, for instance, it is only

[1] The term "alogical" is used in this bulletin for methods of discourse other than logical. The term "illogical" usually implies a defective logical sequence, while alogical sequences are devoid of any sort of logical texture altogether. Conclusions reached by alogical discourse may be true or false, and if true are not demonstrably so without reference to subsidiary verification.

possible to reach a logically valid conclusion if the logical inerrancy of the authority has been first demonstrated. If this is indemonstrable, then the mode of argumentation, though possibly leading to correct conclusions, is lacking in cogency.

An analysis of the Russian literature relating to genetics has brought to light the following modes of alogical discourse. There is, firstly, appeal to recognized authority, secondly, views are criticized if they can be represented as inconsistent with an approved authority—these views will be referred to hereafter as "heretical"—thirdly, analyses are made of the presumed state of mind of the author whose views are under consideration, and fourthly, the truth of a theory is estimated by its practical utility. It is obvious that these categories of alogical discourse are not sharply distinguishable, but it is convenient to consider them under separate headings. It also happens frequently that all four forms of alogical discourse are utilized in a single paper, usually intermingled inextricably with strictly logical reasoning, the whole complex of thought forms presenting a facies highly distinctive of the school of genetics being treated.

Authorities

Arguing from an approved authority is very characteristic of recent genetical authors in the Soviet Union. Berdyaev has gone as far as asserting that "all controversies in the sphere of theory, ideas and philosophy, and all disputes in the practical, political and economic world in Soviet Russia, are fought out under the banners of orthodoxy and heresy". The method of arguing by authority has indeed been explicitly recognized by Prezent[b], who discusses the criteria to be applied in choosing the best authority.[1] The following list includes the principal authorities utilized:—

Dialectical Materialism	Mičurin
Darwin	Burbank
Timirjazev	Lysenko.

Many Russian scientists have reiterated the necessity of basing science in general and biology in particular on the philosophy of dialectical materialism as developed by Marx, Engels, Lenin and Stalin. Muralov, a former president of the Lenin Academy of Agricultural Sciences, commenting on the 1936 genetical conference, emphasized the necessity of basing agricultural sciences on Marxism, and was echoed by Poljakov at the 1939 congress, who expressed similar views in relation to biology in general. Žebrak[c] has been an unswerving exponent of dialectical biology although attempting to show that genetics as commonly understood and Marxism are compatible.

Krenke's theory of Cyclic Development is based explicitly on dialectical materialism, and his outlook on plant physiology is commended by Maximov, whose textbook is well known in its American translation, and who declares that "the theory of development can be successfully worked out only on the basis of dialectical materialism, which Soviet scientists have begun to use more and more successfully as the basis of their theoretical concepts".[2]

Jakovlev[b] in 1940 commended the work of Mičurin as animated by the spirit of Marxism, and later, in 1944, Fersman insisted that dialectical materialism must form the basis of all scientific knowledge.

This constant emphasis on the necessity of basing science on dialectical materialism has not escaped the notice of foreign observers, and drew forth unfavourable comment from the American cytologist Sax,[a] who roundly declared that genetics in the Soviet Union was in a state of subservience to the official political philosophy. That a certain respect is paid in Russia to official statements relating to scientific matters seems certain. Kolman in 1940, criticizing Kolmogorov's[a] defence of Mendelian genetics, stated that "the adherents of Mendel and Morgan will see in Kolmogoroff's article 'a mathematical proof of Mendelism'. This error will be of doubtful service to them, for it will hinder them from adopting the conclusions arrived at at the conference convened by the editorial board of the magazine 'Under the Banner of Marxism', in October, 1939, on problems of genetics and breeding".

Popular support is adduced by Lysenko as evidence of the truth of his theory of intravarietal crossing, the enthusiasm of the collective farm workers being described and extracts from their

---

[1] авторитет.

[2] учение о развитии может успешно разрабатываться только на базе материалистической диалектики, которую советские ученые все более успешно начинают класть в основу своих теоретических построений.

letters quoted. Meĭster,[b] in 1936, notes that the Institute of Plant Industry had been forced to revise its views in deference to Soviet popular opinion, and Muralov in 1937 set up the work of the Odessa Research Institute under Lysenko as a model to be followed by other research institutes throughout the Soviet Union.

The use of Darwin as an authority appears to have become practically universal, a development that was greatly assisted by Prezent's powerful attempts to establish Darwin as the supreme authority in genetical matters. It has been already noticed that Prezent was unable to secure any interest in Darwin's views until 1936, but thereafter, by associating Darwin's biological views with dialectical materialism, Prezent was able to construe any criticism of Darwin as an attack on the materialistic basis of biology. For this reason Darwin's writings have become extraordinarily important in the eyes of Russian geneticists, and are quoted as incontrovertibly authoritative.

This erection of Darwin's work to the status of a canon and the grave distrust with which critics of its contents are regarded is one of the strangest developments of genetical science in Russia. It is impossible to avoid comparing this form of Darwinian exegesis with the extremely literal interpretation of the Bible practised by Christian fundamentalists. In both cases there is an unhesitating *a priori* acceptance of the trustworthiness of a certain set of writings, without the necessity being realized that such trustworthiness itself needs a preliminary vindication.

The number of passages which might be cited as evidencing Darwinian fundamentalism in Russia are so numerous that only a small fraction can be included. Prezent,[a,b,c] as might be expected, is one of the most earnest advocates of Darwin's writings. He accuses his opponents of having fallen into error largely through their ignorance of the Darwinian classics; he maintains that biology must be based on Darwin and permeated with the theories of dialectical materialism; and he goes as far as saying that neutrality to Darwin's writings is impossible, for those not with Darwin are against him. Similar remarks have been made by Lysenko,[j,t] Muralov, Orbeli and by Meĭster,[b] who eulogizes the later materialistic Darwinism developed *par excellence* in the Soviet Union.

Darwin's support is quoted in favour of Mičurin's theories by Jakovlev,[b] in favour of the inevitability of deterioration following self-fertilization by Dolgušin,[a] and in favour of vegetative hybridization by Lysenko.[t] Many other examples could be mentioned, but it is only necessary to refer to one further instance, that of Žebrak, who endeavoured to defend Mendelism at the 1939 genetical congress by suitable quotations from the works of Darwin, thereby attempting to meet his opponents on their own ground.

Reference must next be made to Timirjazev, who has been also canonized by Prezent,[a] and used to criticize the present exponents of Mendelism. Lysenko[t] has taken over without modification Timirjazev's classification of the modes of inheritance, which, although a fair presentment of the state of genetical knowledge in the opening years of the present century, hardly does justice to more recent developments. Žebrak has also tried to use Timirjazev in support of Mendelian genetics, quoting Timirjazev's remark that Mendelism removed some of the original theoretical objections against Darwinism.

Mičurin, the famous Russian horticulturalist, has been accorded an official cultus and is extensively quoted by such authors as Lysenko,[b] Prezent[b] and Kolj. Žebrak tried to adduce Mičurin's support in favour of Mendelism at the 1939 conference, but was taunted by Prezent with having to stoop to such pretences through the inefficacy of his other arguments, Mičurin's real opinion being quite to the contrary. Burbank is also quoted in support of Lysenko's theories, both by Lysenko[t] and Prezent.

It is hardly necessary to add that Lysenko himself has become an authority in the eyes of his numerous followers. The attitude of such writers as Kolj is typical and it is hardly necessary to multiply examples.

The evidence collected above on the use of approved authorities may cause some surprise to western scientists, but it is only fair to point out that a similar tendency, though usually much more masked, is to be found in their own publications. No exception need be taken of course to felicitous quotation from other writers, in fact, such a habit encourages breadth of view, but there is very grave danger that a scientific conclusion may become accepted solely on account of the eminence of its supporters. Such seems to have occurred on more than one occasion in the genetical controversies of the Soviet Union, and, though not disproving in any way the theories being advanced, does not contribute to establishing their validity.

There is also the possibility that the authorities approved may not accord on all points, an instance being furnished by Komarov's quotation of the criticism by Engels of Darwin's excursions into sociology, a discordance hardly resolved by attributing Darwin's opinions on this

subject to the baleful influence of Malthus. Questions of interpretation of the authorities are also important, and English readers of Darwin's works would probably be surprised at some of the constructions put upon his cautious theories by such exegetes as Prezent.[c]

The whole intellectual atmosphere of the recent genetical controversies is strikingly parallel to the situation obtaining in the Middle Ages where argument by authority was also prevalent. It is interesting to notice, however, that in medieval times authority was only invoked, at least in theory, in matters of theology which were believed to be infallibly revealed, while philosophy, although reverencing Aristotle was regarded as a matter for debate. In the Soviet Union, it would appear that philosophy is hardly a matter for debate, one particular philosophy, dialectical materialism, being officially put forward for universal acceptance. Stalin has indeed developed the theory of bolshevik axioms, matters which should be universally accepted by authors and whose validity should be conceded without question. Such axioms, if true, would of course be most helpful, but it would still be necessary to enquire first into their validity, for none of them is self-evident.

Heresies

The term "heresy" has been adopted for those points of view which are regarded by Russian geneticists as inconsistent with their approved authorities. The principal are as follows:—

Metaphysics	Fascism
Vulgar materialism	Abiologism
Capitalism	Lamarckism.
Fideism	

Practically all the criticisms that the followers of Lysenko have levelled against Mendelian genetics involve the charge that the latter is metaphysical, an accusation that will probably cause some surprise to many western biologists. It is necessary to realize, however, that the term "metaphysics" has a special connotation in Marxian literature, signifying neither the study of being in general as in post-Platonic realism, nor obscure speculation involving immaterial entities as in debased modern usage, but merely a type of thought involving illegitimate abstractions transcending the dialectical material reality which Marxian writers agree in accepting as the sum-total of existent being.

Marx, Engels,[a] Plehanov and Lenin all inveigh against philosophical systems in which the existence of immaterial being is admitted, including in particular the idealisms of Berkeley, Kant and their successors. It is therefore not surprising to find Prezent[e] stigmatizing pre-Darwinian biology as idealistic, nor Žebrak hurling a similar charge against the theories of Mičurin. Both Lysenko and Prezent[b] on the one hand, and Žebrak on the other, accuse each other of indulging in metaphysical abstractions, while Prezent[e] insists that Vavilov's Law of Homologous Variation contains a teleological element, i.e. admits the existence of directional mutation irrespective of adaptation to environment, which view is declared to be inconsistent with dialectical materialism. The most frequent charge made by Lysenko[b] and Prezent[e] against Mendelism is that it is formal, i.e. it concerns itself not with the material reality of organisms, but with an illegitimate series of abstractions, the genes, whose existence is unproved and extremely improbable.

Dualism is but a particular type of formalism and is exemplified by Weismann's distinction of living substance into germplasm and soma. Modern genetics is accused of having perpetuated this formalistic dichotomy in so far as a distinction is drawn between the hereditary substance of the genes and the non-hereditary substance of the cytoplasm. It is urged that, according to dialectical materialism, such a postulation of two discrete non-interacting substances is an absurdity. The details of the criticisms raised against genetics on these grounds will be reviewed in Chapter VI.

Lenin, in his *Materialism and Empiriocriticism*, is careful to dissociate himself from such materialists as Holbach and Feuerbach, whom he stigmatizes as vulgar materialists, a term used to describe adherents of materialism who do not accept dialectics. It is therefore not unexpected to find Prezent[c,f] suggesting that the method of factorial analysis is vulgar, involving the interaction of material entities without an acceptance of the dialectics of development. He insists that Mendelism has affinities with the vulgar materialism of Dühring which was attacked by Engels[a] in *Anti-Dühring*. Both systems are condemned on account of their combinationalism, i.e. explaining change and development as the outcome of random combination of subordinate entities, rather than as an essential property of matter as in dialectics. In Prezent's own words, "in such a field, there cannot be any organic interrelationships such as the inheritance

of purely mechanical relations. The formation of the hereditary basis cannot be an interplay of combination, and cannot be the chance play of atom-genes".[1]

The next heresy, capitalism, concerns the environment of the protagonists of modern genetics rather than the content of their theories. Lysenko[1] and Prezent[e] constantly refer to the shortcomings of genetics as caused by the bourgeois mentalities of its supporters. There is a constant implication that ideas are secondary to material conditions whose vicissitudes they reflect. Genetics is regarded, from this point of view, as the inevitable outcome of a capitalist society, and should therefore be replaced by a scientific system more consonant with the communist society of the Soviet Union. Disparaging remarks on the development of genetics in capitalist countries are not wanting in the works of Vavilov,[b] nor is it uncommon to find the charge of contra-revolutionary thinking being raised both by Lysenko and his followers, and also by his opponents.

Fideism, or belief in theism, is regarded by Lenin as a particularly pernicious form of metaphysics, since it involves not only the acceptance of ideas foreign to dialectical materialism, but also invades the spheres of morality and social behaviour. He points out with much indignation that the idealistic trends in the philosophy of science, which were current in the first decade of the present century, were accompanied by a recrudescence of theism quite incompatible with his interpretation of Marxism.

Unfortunately for the reputation of genetics in the Soviet Union, there have been two clerics, Malthus and Mendel, who have played important parts in developing its theoretical ideas. Engels, according to Komarov, attributed what he considered the deficiencies in Darwin's own ideas to his following Malthus, an Anglican clergyman, on population questions, an opinion reiterated by Serebrjakov in 1939. The fact that Mendel was a priest has been similarly used to discredit his ideas. Lysenko[j] repeats Timirjazev's innuendo that Mendelism represents the attempt of clerical reactionary elements to replace the materialistic biological system of Darwin by a veiled form of idealism. It will be remembered that Lenin put forward a similar explanation with regard to Mach, whose idealistic philosophy of science was regarded as a comparable movement by reactionary elements against nineteenth century materialistic physics. It is true that Prezent sometimes excuses Mendel from full responsibility for Mendelism, but this does not prevent Lysenko and his followers from bringing forward on many occasions the accusation that genetics is the product of reactionary clericalism.

When considering the next heresy, fascism, a new facet of contemporary Russian thought is involved, and one that is surcharged with very strong emotional reactions. As early as 1935, Prezent[a,e,f] was arguing, with some cogency, that genetics was the foundation of the German racial theories. He accused Navašin of providing a scientific basis upon which racial theories could be erected, and thus of undermining the theory of social equality which was necessary for the development of a classless society. There is no doubt that Prezent felt himself in this way to be defending a vital element of Marxian sociological theory, pointing out the anti-communist implications that can so easily be drawn from Mendelian genetics. A similar attitude was taken by Kolj, who accused Vavilov of lending support to German fascist theories by the theory of Centres of Origin, and Lysenko, later, made a similar charge against Vavilov at the 1939 genetical congress.

Although still a matter of controversy, there can be no doubt that genetical research has demonstrated the heterogeneity of the human race, and has therefore provided a potential basis for the development of theories of racial and class distinction. It seems clear that Lysenko and Prezent realized these implications and found in them a serious objection to the theory of social equality. The growing political tension between Russia and Germany no doubt served to inflame these suspicions.

Prezent[e] is inclined also to regard the whole genetical controversy as part of the contemporary class struggle. He remarks that "anyone who does not understand the enormous social-class significance of our controversy will fail to understand the essence of our discussion".[2] The identification made between Mendelian genetics and the bourgeoisie on the one hand, and Lysenko's system and the communist society on the other, has been already noted, and has played a significant role in the controversies that have taken place.

---

[1] не может быть в такой области органических взаимоотношений как наследственность чисто механических связей. Формирование наследственного основания не может быть простой комбинаторикой, не может быть случайной игрой атомов-генов.

[2] Тот, кто не понимает огромной социально-классовой значимости нашего спора, тот не понимает и существа нашей дискуссии.

Abiologism, the next heresy which must be considered, is the name given by Prezent[c] to those methods of biological analysis which fail, in his opinion, to take into account the fundamental nature of living organisms. There is much evidence to show that both Lysenko[m,p] and Prezent[b] are extremely suspicious of mathematical analysis in biological problems, both asserting that the material reality of organisms cannot be satisfactorily described in the abstract symbolism of mathematics. Genetics they regard as a prime offender in this respect. It is accused of being an abstract, algebraic projection of the facts, dealing not with real entities but with statistical correlations between characters and fictitious symbols, the Mendelian factors. The whole mathematical treatment of probability appears extremely unpalatable to Lysenko, a point which Haldane[b] endeavoured to rectify in 1940.

The validity of mathematical analysis in genetics was the subject of a controversy between Lysenko[p] and Kolman on the one hand, and the mathematician Kolmogorov,[a,b] on the other. The latter had analysed the figures published by Ermolaeva,[a] a follower of Lysenko, on the segregation behaviour of peas, and which the authoress believed disproved Mendel's laws. Kolmogorov showed that the discrepancies noted by Ermolaeva were not statistically significant, a finding which provoked Lysenko to declare that "we biologists, however, do not want to submit to blind chance, even though this chance is mathematically admissible. We maintain that biological regularities do not resemble mathematical laws".

Lysenko was supported in this protest by Kolman, who asserted that "only on the basis of biology . . . can Mendel's law be proved or disproved as a universal biological law. . . . Mendel's law of segregating factors deduced from a definite group of cases of inheritance is nothing but a statistical rule, and not a universal biological law. It is, moreover, a rule which essentially depends on the mode of classifying factors. Finally, it should not be forgotten that statistics as applied to biology must occupy a subordinate place. According to Engels and Lenin, the higher the form of movement, the more difficult it is to apply to it the mathematical method and the less effective is this method for the perception of reality. For these reasons it is obviously futile to prove or disprove Mendel's law by statistical and mathematical methods".

Kolman's further criticism of Kolmogorov's article is worth reproducing at length, for it illustrates both the distrust felt for mathematical methods of biological analysis, and also the method of associative discourse, frequently alluded to before, in which the views of an author are rejected because of his association with some other author already condemned *a priori*. In this example there is a chain of associative links, Kolmogorov, Mises and Mach, the latter condemned by Lenin for his supposed association with Berkeley, the prime heresiarch of dialectical materialism. Kolman writes:—

"Kolmogoroff's wrong conclusions evidently spring from his methodology, in the question as to the role of the theory of chances and statistics in the study of the processes of the material world. . . . In his analysis of the pre-requisites for the application of the theory of chances to the world of reality, he has to a large extent followed Mises' conclusions. . . .

"We shall therefore turn to the indicated work by Mises.[b] Indeed, on p. 22 of the said work we find a clear-cut formulation of the relation, in which the theory of chances stands to reality according to Mises: 'The theory based on the two axioms—existence of limits and their insensibility to the choice of place—may claim that it is a mathematical theory, that like the theoretical construction of geometry it can only be verified by the laws of reasoning. . . .

"Even those who have but a slight knowledge of philosophy will see what the epistemological roots of this proposition are. But to leave no room for doubt, Mises[a] refers the reader to another work of his which, as he says, reveals the philosophical basis of his principles. In this work, on p. 182, we read that in questions pertaining to language, conception and substance, he, Mises, 'mainly upholds the point of view of Mach' . . . .

"Mises is a true follower of Mach, and his views on the relation of theory to reality are the same as those which were subjected to destructive criticism by Lenin in his 'Materialism and Empiriocriticism'. To follow the view of Mises to any extent (let alone to a great extent) is in no way to be recommended".

This interesting passage, it will be observed, does not at any point subject Kolmogorov's views to logical analysis. Instead, his views are compared firstly with those of Mises, then Mises' with those of Mach; Mach is then condemned by citing Lenin's criticism, Mises condemned for his connexion with Mach, and Kolmogorov for his affinity with Mises. It is possible of course that Kolmogorov's views are open to logical criticism, but Kolman has preferred to use this form of associative alogical discourse in making his point.

A similar argument by associative discourse was brought forward against Vavilov by Prezent[f] in 1939. In condemning Vavilov's Law of Homologous Variation and his theory of Centres of

Origin, Prezent pointed out that Vavilov was a disciple of Bateson, the notorious anti-Darwinian heresiarch. What is more, Timirjazev is stated to have dubbed Bateson an "irresponsible ignoramus, an unbridled scholastic realist and a cleric of science". Vavilov is regarded as having inherited these damnations and his theories are thereby discredited.

The last heresy, Lamarckism, is mentioned principally in the writings of the Russian opponents of Lysenko, who use it with the same logical looseness that disgraces their colleagues in other countries. The term Lamarckism has become so degraded in the course of the last hundred years that it has come to signify little more than the mud slung by neo-Darwinians at other schools of thought. Lamarck's own views have already been treated. His belief in the mutability of species and his theory of directional mutation under the influence of environmental and behavioural factors were adopted by Darwin, who discarded however some of Lamarck's teleological assumptions. Since that time, the term Lamarckism has been applied indifferently to any theory of directional mutation, any belief in the inheritance of acquired characters, or any assumption of teleological and psychic directiveness in evolution. The Russian Mendelists appear to be using the term as a reproach against Lysenko's theory of the plastic genotype, which they contrast to the comparatively stable entity postulated by geneticists generally. Little clarification in the genetical controversy has resulted from such language and it is pertinent to quote Lysenko's remark that a belief in the persistence of artificially induced chromosomal aberrations is itself tantamount to a belief in the inheritance of acquired characters.

Prezent[c] has been at some pains to rebut the accusation of Lamarckism. He distinguishes firstly between psycho-Lamarckism and mechano-Lamarckism, and then asserts that whereas mechano-Lamarkists hold that environmental factors "diffuse" into the genotype, Lysenko on the other hand regards them as becoming "assimilated" by the genotype. His explanation of Lamarckism is not satisfactory, nor his distinction between diffusion and assimilation.

Analysis of Motives

Pre-occupation with questions of motivation has long characterized Russian literature. It has also been a conspicuous element in the Marxian classics, in which an attempt is made to examine ideas, not as regards their logical structure, but in their relationship to material conditions. It is therefore not surprising to discover to what an extent the Russian genetical controversies have hinged around psychological issues.

So varied has this interest been that it is most convenient to treat the matter chronologically. In the first important publication of Lysenko and Prezent, reference is made to the adverse criticisms of Mendelian geneticists, whose motive for criticism is stated to be mere love of objecting. In the following year,[b] Prezent published a long paper on Lysenko's theories and carried the method of psychological analysis to an extraordinary length. He adopts a method of argumentative discourse, much favoured by Lenin, of likening his opponent to one or other character in the classic Russian novels, illustrating the supposed motives of the person concerned by quotations from the descriptions given in the novels. In this way, Prezent compares the methods of Mendelian geneticists with the apocalyptic speculations of the Old Believers described by Saltykov in his novel of the same title. The method of factorial analysis and the arrangement of factors and modifiers to fit the observed segregation ratios is compared with considerable appropriateness to the following description by Saltykov:—

"Among the hermits every honour is paid to the word Antichrist, for it is a world-puzzle in their hands which they solve in many ways. If a letter is wanting for their purpose, they do not scruple to add it. If there is a letter too much, why, they cut it off. If the Russian word does not fit in, they just translate it into Greek; and, if necessary, they add a title, Count, or Prince, or Imp of Darkness. These calculations go on till at length the meaning of Antichrist appears. The simple folk are much impressed by these reckonings".

On another occasion, Prezent[e] compares the discussion between Morgan and Punnett on the genetic factors determining the inheritance of feather colour in fowls to the discussions between the medieval nominalists and realists on the mode of existence of universal ideas, both controversies, according to Prezent, being unreal, concerned solely with illegitimate abstractions and scholastically sterile.

Prezent[b] also attacks, with considerable justice, the tendency of biologists to invoke Latin or Greek terms in order to mask their ignorance of the problems they are discussing, and he asserts, again not without reason, that the popularity of factorial analysis is owing partly to the fact that it is far easier to juggle with segregation ratios than to conduct investigations into the comparative physiology of development. He deals particularly sharply with the unpublished article, already mentioned, by Žebrak, whom he accuses of writing as he does through anger at the

contempt shown to genetics by Lysenko's school, and through indignation that people of humble origin should presume to criticize the dicta of a closed society of self-regarding geneticists. Žebrak's remarks that the prediction of dominance is of little importance in practical agriculture is taken to imply that Žebrak is chagrined at his inability to make such predictions for himself.

In the same year and later, Lysenko[b,g,k] uses the same method of psychological rebuttal in meeting the criticisms of his theories made by Vavilov[a] and Konstantinov *et al.* He suggests the possibility that Vavilov's claim to have obtained transgressive segregation for earliness in flax may be a false claim merely to embarrass his opponents. With regard to Konstantinov *et al.*, Lysenko[j] declares that ill-will is the main motive for their criticisms and a desire to bring the Odessa Institute into disrepute. Such objections based on presumed unworthy motives are declared by Lysenko to necessitate no direct reply. He adds that his own articles "although they are passionate are in all cases impartial".[1] His opponents' articles, on the other hand, are condemned as "passionless, cold-blooded and measured, but nevertheless partial in the extreme".[2]

In 1937, Prezent[e] continued his psychological studies and tried to show that Vavilov was only using Darwinian phrases in order to conceal his anti-Darwinian bias. Konstantinov was accused of not having the yields of socialistic agriculture at heart. Examples of such treatments from Prezent's writings could be multiplied at length, but it will suffice here to cite a few examples from other authors.

In 1939 Maštaler accused Mendelian geneticists of conspiring to vitiate Darwinism. Kolman, whose criticism of Kolmogorov's article has already been noted, accused the latter of intriguing with Serebrovskiĭ, who, in spite of apparent denials, is declared to have been endeavouring to marshall opposition to the ideas of Darwin and Mičurin. He adds that the reason why genetics has been so generally accepted is that "owing to the extreme simplicity of the admissions on which it is based, Mendel-Morgan genetics, representing an apparently practical domain, leaves nothing to be desired as a field for mathematical exercises".

These few instances out of the many that could be cited are sufficient to show what importance is attached in the Soviet Union to psychological issues. Such an interest does not of course detract from the value of the strictly scientific analyses that are made, but there is a danger that psychological pre-occupation may mask the real points at issue, those of the scientific validity of the theories being considered.

The Pragmatic Test

Lenin, as is well known, was a determined opponent of the philosophy of pragmatism which was rife in the first decade of the present century. His objection was not based on the practical consequences of the theory but on its idealistic background. Both Marx and Engels[a,b] emphasized that dialectical materialism was a philosophy of practical activity and that pure science should be discountenanced in favour of science applied to the needs of society. This point of view has been stressed recently by Fersman in Russia and Bernal[b] in this country.

It is not surprising therefore to discover a sense of practical urgency underlying the whole of the Russian genetical controversy. The need for breeding new varieties in the shortest possible time and of increasing crop yields to the uttermost is realized by both sides, and there is a tendency consequently to judge the truth of genetical theories by their supposed practical utility.

Mičurin's admirers Jakovlev[a] and Stepanov defended his theories as efficacious in practice, while one of the chief props of Lysenko's theories from the time of their first introduction, has been their claim to practical utility. Constantly in the early work of Lysenko[b,c,h] and Prezent[b] the claim is made that, by using Lysenko's theories, a new variety of crop plants can be bred in a period of two and a half to three years, instead of the much longer period needed by Mendelian geneticists. This claim is reiterated in many of the papers by Lysenko and Prezent, until, by 1939, the period required for a new variety had dropped to one or two years.

The general practical usefulness of Soviet Darwinism was affirmed by Eĭhfeljd in 1939 and the claims of Mičurin and Lysenko that the hereditary constitution of plants could be modified in favour of social needs were used as further evidence of their validity.

It is true that doubt has been expressed from time to time as to whether these claims are in fact genuine, as by Konstantinov *et al.* in 1936, who stated that some at least of Lysenko's varieties were inferior to the standards and had not been rigorously tested in the state variety trials. These remarks, however, did not appear to discomfort Lysenko and his followers, who

---

[1] хотя и являются страстными, но во всяком случае беспристрастны.
[2] нестрастны, хладнокровно-размерены, но зато сугубо пристрастны.

have always insisted that their practical achievements are cogent evidence in favour of their theoretical notions.

How far these claims can be accepted and what light they throw on the validity of Lysenko's theories will be considered later.

Logical Discourse

Having dealt quite briefly with the various forms of alogical discourse practised by Russian geneticists, it is necessary to refer to the pecularities of their logical discourse. It has already been suggested that the minor role played by the Russian universities may have contributed in part to the comparatively large part played in the genetical controversies by alogical forms of disputation. Some of the traits to be mentioned next may also be due to the same cause.

There is firstly a very confusing use of terms, which led Žebrak to observe that many of Lysenko's expressions were quite untranslatable into ordinary genetical terminology. Prezent[b] however was able to point out that this difficulty was due, not perhaps to the obscurity of Lysenko's terminology, but to the deficient and formalistic vocabulary of genetical science. Even though there may be some justice in this reply, it is undeniable that Lysenko's use of terms is frequently very obscure. Added to this difficulty is the fact that Lysenko and Prezent are constantly putting forward fresh definitions of such standard terms as Darwinism, genotype, assimilation, nutrient and pure line. Frequently two unequivalent definitions of the same term are given in the course of a single paper and it often happens that the meaning attached to a single term fluctuates throughout the length of a paper and perhaps is not explained at all. Some of the definitions given by Lysenko seem to bear practically no relationship to the normal meaning of the term. For instance, heredity is defined on one occasion as "the capacity of the living body to demand definite conditions for its life and development and to react in a definite way to any given conditions",[1] an obscure definition having little in common with the customary connotation of the word. Lysenko fortunately does not usually appear to follow this definition himself when speaking on questions of heredity.

Similar confusion surrounds Prezent's[c] definitions of the term "genotype", which is sometimes used as synonymous with individual genetic nature and sometimes for the common properties of a group of organisms. The term "requirement"[2] has led to much misunderstanding. In Russian, the word has a somewhat vague connotation and may in ordinary speech have the conative meaning of "demand", or may imply nothing more than a necessary condition. M. M. Zavadovskiĭ has criticized Prezent's use of this word since he asserts that it carries teleological implications. This is perhaps rather overstating the objection. Prezent's habit of taking words in common speech and endowing them with technical meanings other than their current significance is indeed regrettable and a source of confusion. But it is only fair to realize that the meanings of the words so modified have changed, and it does not follow that because a Russian word normally carries a teleological implication, it will do so when used by Lysenko and Prezent. The reason why Lysenko and Prezent make use of non-technical words with technical significations appears to be their eagerness to enlist the sympathy of the farm workers for their genetical theories.

A further difficulty in the writings of Lysenko and Prezent is provided by the syntactical incoherence of many of their sentences, another point criticized by Žebrak. This point is particularly apparent in the disconformity in their sentences between subject and predicate, the two often belonging to quite different orders of things and having no obvious connexion. The anomolous forms of predication used are one of the most serious difficulties in the way of understanding the true significance of Lysenko's theories, and have helped to give rise to the erroneous impression that Lysenko's theories are no more than fantasy.

In matters of logical sequence, both Lysenko and Prezent, especially the latter, are able to argue with great dexterity and keen discernment of the points at issue. This is particularly evident in their criticisms of certain current Mendelian notions whose weak points they expose with considerable acumen. It is important to remember, however, that both authors are addicted to sarcasm and may argue ironically for many paragraphs on end, sometimes deceiving the reader into supposing himself mistaken as to the point of view being taken by the article, until at length the author resumes an unequivocal style.

It is in critical analysis that Lysenko and Prezent are seen at their best, although even here

---

[1] свойство живого тела требовать определенных условий для своей жизни, своего развития и определенно реагировать на те или иные условия.

[2] требование.

there is a marked tendency to criticize a theory by seizing some subordinate aspect, often non-essential to the whole problem, and tearing this to pieces. This is frequently done with dramatic effect and the reader is left with the impression that the theory criticized has been utterly demolished, whereas the main point has, often as not, been completely avoided. There is also a tendency to parody ideas before criticizing them, and then subjecting the parody to critical demolition, a matter which is seldom difficult.

One of the weakest points in the critical analyses of Lysenko and Prezent is their ignorance of the relevant literature, a fault which has been repeatedly stressed by such geneticists as Meĭster[a] and Žebrak. Prezent[a] defends himself by the counter-accusation that his opponents are ignorant of Darwin's writings, a charge that hardly neutralizes the first.

In their constructive logic, Lysenko and Prezent are not seen to best advantage, since both authors grossly oversimplify the complexities of their problems and fail to recognize the many alternative explanations that could be proposed in lieu of their own suggestions. On more than one occasion, Lysenko and Prezent declare that their genetical theories are a logical development of Lysenko's theory of phasic development, a statement that few logicians would be prepared to admit.

Still less satisfactory are the methods used by Lysenko and Prezent in defending their theories from the counter-attacks of Mendelian geneticists. In 1936, Konstantinov *et al.* accused the workers at the Odessa Institute of ignoring any facts discovered at their own or other institutes which conflicted with Lysenko's theories, a matter that came to the fore after Maksimčuk had revealed that Lysenko had deliberately refrained from alluding to experiments performed at Odessa in which F_1 hybrid plants had been obtained later than the earlier parent. Lysenko had an explanation to cover this discrepancy, but his failure to mention the matter until forced to do so created an unfavourable impression.

The mode of dealing with discrepancies adopted by Lysenko is not altogether satisfactory. Discrepant facts are termed bare facts,[1] meaning facts with the relevant conditioning factors unspecified. Lysenko claims that, were the relevant conditions specified, then the fact would be found to conform to his theories, an assumption that seems in many cases gratuitous. In addition, Prezent[a] riposted by accusing Mendelian geneticists of ignoring the evidence published both by Darwin and by Lysenko and his followers. He accused geneticists of unwillingness to repeat the basic experiments on which Lysenko's theories were based, a charge that even today is not without foundation.

IV. THE EVIDENCE

It might be supposed that the genetical controversies of the U.S.S.R. are concerned not with questions of fact but with problems of interpretation. This is not the case. There are as much doubt and controversy over matters of fact as over the knottier problems of explanation.

The Russian journals contain a large bulk of experimental results which are of great value in analysing the theories put forward, but there are, unfortunately, many reasons why extreme caution must be exercised before accepting these as they stand. There is, firstly, some doubt as to the technical efficiency of the various experimental procedures described. Vakar, for instance, criticizes Lysenko's method of emasculation and declares that it is not 100% effective. Control experiments are not always mentioned in experiments designed to demonstrate various forms of hereditary behaviour, and even when made, these are not always as carefully set up as scientific precision demands.

Rather more serious is the attitude of Lysenko and his followers to mathematical methods, which has already been noted in the previous chapter. Since much of the evidence he adduces is quantitative in its nature and deals with population samples, it is essential that such experiments should be analysed mathematically. This is not to assert of course that all aspects of biology are adequately treated by mathematical methods, but it is certainly necessary, when considering quantitative data, to use the appropriate mathematical means of analysis. A particular antipathy appears to exist in Lysenko's school to the mathematics of probability

---

[1] голые факты.

and statistical analysis, a particularly unfortunate distaste since it is quite impossible to assess the significance of many of its experimental data without statistical analysis.

Kolmogorov's[a] invalidation of Ermolaeva's[a] claim to have disproved Mendelism is a case in point, and earned the opprobrium of Lysenko[p] and Kolman. The latter authors maintained that biological laws could neither be proved nor disproved by mathematical analysis, a statement not completely accurate. It is certainly impossible to prove a biological law by purely mathematical methods, but it is possible to show, if the law is quantitative, whether its numerical statements are concordant or not with the observed facts. Regarding the second half of their claim, it is possible to be more definite. They are in error in supposing that mathematical analysis cannot disprove a biological law. If such a law is quantitative, its numerical statements are susceptible to mathematical analysis, and if it is shown thereby that the quantitative aspect of the law is unsound, then the whole law is disproved, since it is only necessary to disprove part of a statement to invalidate the whole, even though the other parts of the statement taken alone may be accurate in themselves.

The extreme heterogeneity of Russian genetical material is another reason why great care must be exercised in analysing the experimental data. Dolgušin[c] reports that the wheat Krymka, much used in Lysenko's genetical experiments, is highly variable, and a similar suspicion rests upon many of the other Russian varieties, which appear to be characterized by little of the uniformity usual in English and American crop varieties. This heterogeneity is of course another reason why statistical methods are necessary in evaluating the results of Russian genetical research.

An instance in which a lack of statistical analysis has prejudiced debate is provided by Lysenko's[b] discussion in 1935 of the mode of inheritance of maturation times. He declared, in reply to Lepin and Maksimčuk, that differences in earliness of two days in one case and one to two weeks in another were not significant in a pure line. The whole question at issue however turns on this very point and the lack of precision in his statements, caused by failure to use statistical methods, vitiates his whole treatment.

Difficulties are also caused by the small number of experiments on which Lysenko builds his theories, and on the small scale on which many of these are carried out. Žebrak alludes to the first point, and Prezent[b] himself, in 1936, reveals that in his hybridization experiments, Lysenko never requires more than two square metres of greenhouse space for selection purposes. This meagre requirement for space is eulogized as an economical desideratum and is compared favourably with the far bigger demands on space made by Mendelian breeders.

A final difficulty that dogs anyone endeavouring to ascertain the data upon which the Russian theories are based is the fact that accusations of fraud are not infrequently made. Authors may possibly make allegations without meaning to be taken seriously, but all the same, they cannot but increase the hesitancy of the geneticist trying to form an unbiassed picture of the factual evidence.

It might be thought that evidence in favour of Lysenko's theories could be drawn from the practical achievements to which he so frequently alludes. The claim of being able to produce valuable new varieties within a period of three years was one of the most dramatic announcements of Lysenko when he first brought out his new theories. Furthermore, the boast of being able to bring about desirable changes in the hereditary constitution of cultivated varieties presumably implies that such experiments have been put to practical use.

An examination therefore of the varieties at present cultivated in the Soviet Union should help to elucidate the value of the many claims already mentioned. Unfortunately, the evidence to hand on this point is too scanty to justify a definite conclusion. Lysenko's new varieties do not seem to be mentioned in agricultural reports to the extent that might have been expected, but whether this is due to lack of relevant information, or whether in fact these varieties have not been widely planted is as yet impossible to decide.

The strength of the factual basis of science has always been its repeatability. In the sections to follow, many unconfirmed results will be mentioned which can neither be accepted nor denied as things stand now. It is only by repeating many of these experiments under strict experimental control that many of the obscurer issues of the Russian genetical controversy will be resolved.

In the sections following, the evidence for each tenet of Lysenko's system will be presented, as far as possible in the chronological order of its appearance.

The Genetics of Earliness

The first controversial genetical theory to be introduced by Lysenko[b,f] was the assertion that "the F_1 produced from crossing any pair of parents cannot in the main be later in flowering

than the earlier parent".[1] It could be as early as the earlier parent or earlier but not later. This was followed by a rider put forward by Lysenko and Prezent that "in none of the following generations can a form earlier than the F_1 itself develop",[2] i.e. transgressive segregation was declared impossible, the F_2 plants being of necessity as early as the F_1 plants or later, but not earlier.

These claims, which were derived largely from *a priori* theoretical considerations, were exemplified by a series of intervarietal cereal crosses which had been effected by Lysenko and his co-workers at the Odessa institute.

Naturally these claims were received with considerable suspicion by Russian Mendelian geneticists, who pointed out that numerous examples of F_1 hybrids later than the earlier parents were known, also instances of transgressive segregation for earliness.

Lepin claimed that transgressive segregation for earliness was frequent in wheat crosses, and quoted an experiment in which the wheat variety Prelude, with a maturation period of 38 days, when crossed with an early Siberian wheat maturing in 43 days, gave rise to F_1 plants with a maturation period of 40 days. These examples were followed by others investigated by Žebrak[c] who obtained transgressive segregation for earliness in the F_2 hybrid progeny of the cross of the pea varieties American Wonder and Giant. Many other geneticists, including Meïster,[a] Konstantinov *et al.* and Vavilov,[a] brought forward similar evidence. Later proof of transgressive segregation for earliness was provided by Svinarev[a,b] for intervarietal crosses of *Triticum vulgare* L. and interspecific crosses of *T. vulgare* and *T. durum* Desf.

The reply made by Lysenko[b] to these objections throws considerable light on his theory, and shows that his opponents had not grasped its full subtlety. He makes a few detailed objections to the data brought forward, claiming that the two days' maturation period of Lepin's wheat hybrid was not significant, and that, in other cases, the observations were not made under strictly comparable conditions.

His main point, however, was that the facts adduced by his adversaries were bare facts, that is, did not specify the external conditions under which the experiments had been made. According to Lysenko's theory of phasic development, an important distinction has to be drawn between environmental conditions in general and conditions of existence[3] in particular, the latter representing those specific environmental factors required by the plant so that it may pass through the various stages recognized by the theory of phasic development.

Lysenko and Prezent claim that all true instances in which either the F_1 plants are later than the earlier parent or the F_2 plants exhibit transgressive segregation for earliness, are to be attributed to the fact that appropriate conditions for existence have not been provided. Their theory, it is claimed, is valid when these conditions of existence are present, but in their absence, deviations are to be expected.

This ingenious explanation has a serious drawback. Lysenko and Prezent offer no means of determining what are, in any particular circumstances, the appropriate conditions of existence. It is assumed that all exceptions to their law are to be explained as due to lack of the appropriate conditions, which explanation is obviously of such a nature that it will cover all instances of crossing, quite irrespective of whether the law is true or not. It would always be possible for Lysenko and Prezent to aver that a particular cross, which failed to follow their law under all the different circumstances tried, would still do so under the correct environmental conditions were they known. Their method of explanation trades on the unknown, and since the demonstration of a universal negative is impossible, their theory is logically irrefutable.

This by no means implies that it is satisfactorily established. The mere fact that it is not possible to predict the requisite conditions of existence for any particular plant vitiates the law. As it remains at the moment, it is an ingenious explanation of the facts, worthless because it fits the facts irrespective of whether it is true or not. The impossibility of deriving the truth value of the law, as well as its appeal to the unknown, render it useless as an item of scientific value, unless these two objections can be removed by further research.

Prediction of Dominance

One of the most frequently reiterated objections of Lysenko and Prezent to Mendelian genetics is the failure of the latter to predict before crossing whether a particular gene will be

[1] F_1, полученное от скрещивания любой пары родителей, в основном не может быть более позднеспелым по цветению, чем ранний из родителей.

[2] Ни в одном из следующих поколений не может развиться более скороспелая форма, нежели самое F_1.

[3] условия существования.

dominant or recessive. Lysenko, on the other hand, insists on the truth of Mičurin's theory that those characters are dominant which are best adapted to the local environment under which the hybrid is reared. A corollary of this theory is a belief in dominance reversal, which has engaged considerable attention on the part of Russian geneticists.

There appears to have been some misapprehension in Russia as to the attitude of Mendelian geneticists to the latter phenomenon, which is assumed to conflict with Mendelian theory. But since cases of this type have long been known and have been comfortably accommodated in modern genetical theory, it is not necessary to quote the relevant Russian evidence.

The main theory, Mičurin's law of the dominance of locally adapted characters, is on a different footing and requires careful examination. Such dominance, on the ordinary laws of probability, would be expected to occur quite frequently, and it is well known that wild type genes are usually dominant to the derivative mutations. What truth there is in Mičurin's theory seems attributable to this. Nevertheless, instances of dominant lethal and semi-lethal factors are known, also cases of recessive factors better adapted to the environment, in which the plant carrying them occurs, than the dominant allelomorphs. The many known cases of fixed dominance under different environmental conditions are also hardly consistent with Mičurin's generalization, and it is also not clear how this law is to be applied to factors of neutral selective value.

These objections are countered in part by the proviso of Tatarincev and Silantjev that the law is operative only in the early stages of seedling development. This explanation of dominance as the expression of juvenile adaptability is used by Prezent[b] to cover the numerous cases of fixed dominance in the mammals. He asserts that dominance in these cases is independent of the external environment, since the controlling conditions are the uniform milieu of the uterus. But by pushing back the time at which environmental influences are believed to be effective to the early stages of ontogeny, there is a danger of employing an elastic[1] hypothesis explaining all apparent exceptions by an appeal to unknown factors in the early stages of development. Such theories, as already explained, cannot be disproved, but their utility is extremely slight and their truth value indeterminable.

To establish Mičurin's theory, it would be necessary to plan a large series of experiments with plants exhibiting dominance reversal, and to correlate the expression of dominance in the various instances with the adaptability of the hybrids to a series of controlled environments. This has not been attempted. There are a few interesting experiments on the manifestation of dominance reversal in individual species, as in Kosmodemjjanskiĭ's experiments with tobacco, but usually single cases are quoted of examples supposed to conform to Mičurin's law, with no reference to the behaviour of plants as a whole. Kovarskiĭ grew hybrids of *Vigna sinensis* Endl. x *V. Catjang* Walp. in two lots, one sown early and the other late. In the first, dark green leaf coloration was dominant and in the second bright green coloration. Similarly, in a comparison between hybrids sown on dry and irrigated land, dark mottled testa colour was found to be dominant in the first case, and yellow coloration in the second. These characters were interpreted as having an adaptive significance and as exemplifying Mičurin's law.

Pjatnickiĭ conducted hybridization experiments with oak species and effected the following crosses: *Quercus Robur* L. x *Q. borealis* Michx var. *maxima*, *Q. Robur* x *Q. macranthera* Fisch. et Mey. and *Q. Robur* x *Q. macrocarpa* Michx. In all cases the progenies obtained resembled *Q. Robur*, the local species, and it was concluded that these crossings conformed to Mičurin's law. Medvedeva in 1944 investigated several cases of interspecific hybridization in wheat. The following combinations were examined: *Triticum dicoccum* Schrank x *T. persicum* Vavilov, *T. dicoccum* x *T. turgidum* L., *T. durum* x *T. persicum*, *T. durum* x *T. turgidum* and *T. persicum* x *T. turgidum*. The F_1 hybrids were each sown in three lots, in February, April and June, respectively. It was reported that the phenotypes obtained thereby varied with the sowing time in such a way that those characters were dominant which were best adapted to the conditions determined by the date of sowing.

Such isolated instances, even though confirmed, would hardly suffice to establish a general genetical law. Only an extended series of experiments could possibly demonstrate the validity of this generalization and even were this done, the facts already known which appear to conflict with Mičurin's statement would require explanation. It is possible, as already mentioned, to formulate the law in such a form that it can be applied to all apparent exceptions by assuming a hypothetical action of environmental factors in the very early stages of development, but such an extension would hardly contribute to the sum-total of genetical knowledge.

[1] The term "elastic" is used in this bulletin for methods of explanation which can be extended to cover any body of facts, regardless of whether the explanation is true or not.

Degeneration of Pure Lines

Lysenko's[b,h,l,m] theory that pure lines on selfing necessarily deteriorate in respect of vegetative vigour is one whose Darwinian roots are very clear. Darwin's[c] own belief in the dysgenic results following selfing have already been discussed in the second chapter. They were adopted by Timirjazev, who regarded this proposition as constituting Darwinism *in sensu stricto*, and they were taken over very early by Lysenko and Prezent. Again, the reasons that led Lysenko to this theory were to some extent *a priori* and consequent upon his general theoretical position. He quotes Darwin's own evidence and also an argument taken from Darwin, to the effect that self-fertilized varieties of plants never persist in cultivation on any large scale for as long a period as 30–50 years, while cross-pollinated varieties of crops, such as rye, are much longer lived.

Lysenko himself does not contribute much in the way of concrete evidence on this point, although he lays great emphasis on the ill effects that attend inbreeding in habitually cross-pollinated species. Dolgušin[a,c] and Sizov support Lysenko's views, though more by argument than by experiment. Krenke has also stated that loss of vigour in pure lines is quite in accordance with his theory of Cyclic Development.

Opposition to Lysenko's view was early formulated by Meĭster,[a] who denied any necessary connexion between inbreeding and deterioration. He was seconded by Vavilov,[a,b] who pointed out that several varieties, in particular some of the English cereal varieties, had maintained themselves for very considerable periods without decline. / He also pointed out that one at least of the reasons why self-pollinated varieties were continually being replaced was that breeders had devoted more attention to their improvement. Cross-bred varieties are regarded as less susceptible to rapid improvement by breeding methods.

None of the disputants unfortunately were able to produce much in the way of quantitative evidence for or against varietal deterioration, in which connexion it is interesting to examine the figures on the yield of several standard American wheat varieties which have been published by Harrington. He shows that there are strong grounds for believing that these varieties have declined in yield over a period of some years, an effect which he attributes to the gradual elimination of heterozygosity in respect of the factors concerned with yield. Bolsunov published some critical researches into the effect of inbreeding in *Nicotiana rustica*. He claimed that degeneration did not occur and that intravarietal crossing between members of inbred lines led to no increase in vigour. Different inbred lines, when crossed, frequently exhibited heterosis.

It is only fair to admit that a satisfactory account of the genetic control of vegetative vigour has not yet been produced, especially as regards the phenomena of heterosis and inbreeding depression. No attempt will be made here to consider the various theories that have been suggested to explain the facts, since the point at issue is, not the explanation of these occurrences, but the question of fact as to whether inbreeding is necessarily deleterious in habitually self-fertilized plants.

It is true that crossing frequently results in increased vigour in normally self-pollinated species, but whether deterioration always follows inbreeding in these cases is another question. The evidence produced that inbreeding is frequently deleterious in habitually cross-pollinated plants may readily be conceded.

The argument drawn from the impermanence of self-pollinated varieties is not satisfactory, firstly because the efforts of plant breeders to improve varieties have been ignored, and secondly, because Lysenko and Prezent have assumed that named varieties are in fact equivalent to pure lines. There is abundant evidence that many Russian varieties are extremely heterogeneous as Dolgušin[c] admits, and there are also sufficient grounds for supposing that even the highly bred English and American varieties are genetically heterozygous, at least in respect of physiological characters. This being so, evidence of a reduction in the yields of varieties, as supplied by Harrington, in no way demonstrates deterioration in pure lines, but merely begs the question as to whether pure lines are in fact involved.

The confusion caused by equating named varieties and pure lines has obscured many points in the Russian genetical controversy and has prevented the more fundamental issues from becoming obvious.

The evidence provided to date of necessary deterioration in pure lines is not compelling and would require far more stringent experimental observation for its demonstration than it has received so far.

Rejuvenation

Having satisfied himself that pure lines deteriorate, Lysenko went on to suggest a practical means of countering this undesirable trait in economic crops. This was his method of

rejuvenation, or the renewal of the hereditary basis[1] of the variety. According to Lysenko's theory, pure lines are genetically plastic and every plant tends to become adapted to the precise conditions under which it grows. On hybridization, it is believed that the F_1 combines within itself the range of adaptability of each parent, thereby being superior to either in agronomic performance. Lysenko[f] declared in 1936 that "any hybrid arising from morphologically similar parents, whose behaviour in the field is identical, will yield more than either of the parents".[2] As examples, he quoted the F_1 wheat hybrids of Melanopus 0122 x Melanopus 069 and Lutescens 153 x Lutescens 062. In the first combination, the hybrid yielded 17.3 centners[3] per hectare in contrast to 13.0 and 14.2 in the case of the two parents respectively. In a second replication the corresponding yields were 18.6, 15.7 and 14.7. The yield of the second hybrid was 18.0 centners per hectare, the parental yields being 12.8 and 14.2 respectively.

Dolgušin[c] goes even further and claims that by dividing a single plant into two halves and growing these under different environments, it is possible to interpollinate them and obtain manifestations of hybrid vigour. Lysenko[b] insists that, strictly speaking, pure lines do not exist, for their genetical variation is so considerable that even the offspring of a homozygous parent soon become genetically diverse. Using the term however in a loose sense, he claim. that the tendency of a pure line to degenerate may be prevented by his method of rejuvenations

The technique in the case of a cereal variety is as follows. Ten individuals of the pure line are selected and emasculated. A pollen mixture is then made from a hundred or so plants of the same pure line and this is applied to the styles of the selected plants. It is then believed that selective fertilization occurs (see Chapter V), and that the ovum selects for fertilization that pollen grain which would produce the best adapted progeny to the environment under which the pollination is effected. This artificial pollination is performed in spring and may be expected to produce about a hundred grains. These are sown immediately and should produce about two thousand grains in the autumn. By two sowings in the following year, the first in March and the second in July, 2.5 centners of grain are obtained, which in one further generation will give fifteen metric tons of grain sufficient to sow 150 hectares. This technique, which is recommended to the workers on collective and state farms to be performed by themselves, is regarded as equivalent to breeding a new variety within a period of three years. It is not clear how often this rejuvenation procedure is required, but an interval from five to seven years between successive rejuvenations is suggested.

The evidence published on the yields produced by intravarietal crossing in the way described is very voluminous and interesting. Most of the earlier work was done with wheat, Lysenko[d] reporting significant increases in the yield of the variety Girka. In 1938, Lysenko and Dolgušin asserted that the response to intravarietal crossing in the varieties Krymka and Ukrainka varied according to environmental conditions and also with the variety. Dolgušin[b] published further information on the behaviour of the variety Krymka in 1939, and claimed an increase not only in yield but also in winter hardiness, claims repeated by Lysenko[q] in the following year. Meljničenko and Tregubenko found that increases in thousand-seed weight and vegetative vigour, increased winter hardiness and earlier maturity followed intravarietal crossing in the wheat varieties Hostianum 0237 and Tenmarq, the former variety producing an increased grain yield of 1.75 centners per hectare and the latter 1.64. Kargopolova and Ščerbina have investigated the effect of rejuvenation on resistence to *Tilletia* sp. The variety Tulun ZA/32 showed a decline in maximum infection from 65% to 40% while the variety Garnet showed a corresponding decline from 35% to 15%. Yield increases in Tulun ZA/32 and the variety GDS75 were also reported. Other authors who have found yield increases in cereals after intravarietal crossing include Krotov[a] and Bassarskaja *et al.*

The plant investigated most thoroughly after wheat is the tomato. According to Kuljčenko, the varieties Bison and Best of All give increased yields after rejuvenation up to 13.6–40.9%, as well as producing more vigorous plants, and accelerating earliness by two to nine days. Breżnev[a] reports increases in vigour and fruit yield, and an increase in total height of plantsof 11–21%. Alpatjev, in a more detailed investigation of the effects of intravarietal crossing in the variety Best of All, reports increased vigour and an advance in time of maturity of up to two days, also yield increases up to 90%. He notes that these effects appear to depend markedly

[1] обновление наследственной основы.

[2] любой гибрид, произошедший от морфологически неразличимых друг от друга родителей, поведение которых в поле одинаково, будет более урожайным по сравнению с любым из родителей.

[3] 1 centner = 100 kg.

on environmental conditions; the F_2 generation was less productive than the F_1, although still superior to the original parents, but by the time the F_5 had been reached, the effects of rejuvenation had disappeared. Šilova in 1941 obtained yield increases following intravarietal crossing in the tomato varieties Best of All, Erste Ernte, Market Wonder, Early Market and Danish Export. The F_2 and F_3 generations were less productive than the F_1 although more so than the controls.

A few experiments with other species of crop plants will be mentioned briefly. Jakovuk[c] has obtained increased yields from intravarietal crossing of the tobacco varieties Samsun 57, Tyk-Kulak 92 and Tyk-Kulak 235; American 572 and Tyk-Kulak 92 showed increases in earliness. These results are interpreted as consequent on the variability within these varieties in respect of physiological characters, and are believed to be related to the age of the varieties. An anonymous[b] report in 1940 claims that intravarietal crossing in the flax variety Victor results in increase of fibre yield of 13% and an increase in the proportion of long fibres of 13.1–15%.

Šakurov and Mogileva have investigated pea varieties, the former reporting an increase in the number of seeds per pod when the variety Capital is treated, while the latter reports increases in vegetative vigour and the tendency to branch. The egg-plant has been investigated by Averjjanova who found yield increases of 18.2–156.8%, larger and more numerous fruit and a retardation of the withering of the plants at the end of the season. Cinda has published the results of investigations on cotton, in which the varieties Maarad, 1276I, Pima and 2I3 of *Gossypium barbadense* L. gave yield increases of 1.45–5.44 centners of raw cotton per hectare, following intravarietal crossing. Comparable results were obtained for the varieties 36 M2, 8517 and 1306 of *G. hirsutum* L. These effects persisted into the F_2 generation. Increased yields following intravarietal crossing in cotton have also been reported by Ter-Avanesjan.

Other references to the beneficial effects of intravarietal crossing are very numerous. Lysenko[c,e,i,l] has published defences of the method against the criticisms of Meĭster[a] and Konstantinov *et al.* and other papers of interest have been published by Rjabov, Hitrinskiĭ, Malahovskiĭ, Dmitriev and Dolǵusin.[c] Krenke has also accommodated these results in his theory of Cyclic Development.

It is unfortunate that investigations on intravarietal crossing have been chiefly confined to the Soviet Union. Harrington in America, however, has published some useful data on wheat. He showed that significant yield increments followed intravarietal crossing in the varieties Reliance and Apex, but not for the variety Marquis.

Although the absence of statistical methods deprives the Russian work of much of its usefulness, there seems little ground for denying the main contention of Lysenko that intravarietal crossing may under appropriate environmental conditions lead to significant increases in yield. Nor is this result at all surprising when it is realized that most cultivated varieties of crops are probably genetically heterogeneous for physiological factors, especially the Russian varieties which are frequently heterozygous for morphological characters as well. The yield increases observed following crossing are quite comparable to the instances of heterosis known in wider crosses, and the decline in vigour in later generations is paralleled by the similar behaviour of the progeny of heterotic hybrids.

Where it is not so easily possible to agree with Lysenko is in his assumption that cultivated varieties are pure lines and that intravarietal crossing is necessarily between members of the same pure lines. The theory that genetically similar members of a pure line can cross to give heterotic offspring has not been proved, and would require careful experimentation for its demonstration. For the present, the results published by Lysenko and his associates cannot be construed as referring to pure lines, although they are interesting in as far as they demonstrate the amount of variability within named varieties.

Induced Mutation

Darwin's[b] theory that the genetical constitution of organisms changes in direct response to environmental factors was accepted by Mičurin and Burbank and was soon adopted also by Lysenko[l,m] and Prezent.[d,f] Mičurin, as already mentioned, developed the theory that the hereditary constitutions of F_1 hybrid seedlings were especially prone to the effects of environmental factors, a theory that he developed before the October Revolution. Mičurin's theory was defended later by such authors as Isaev, Silantjev, Odincov, Gurov, Sludskiĭ, Gumenjuk and Podufaliĭ, Jakovlev,[b,c,d,e] Usatov, Tatarincev and Silantjev, most of whom were horticulturalists following in the footsteps of Mičurin and applying his breeding methods.

Lysenko[t] adopted Mičurin's theory and extended it to cover a much wider field, in particular his experiments with vernalization. His views on this matter are collected together in his

paper on heredity published in 1943, in which he presents what he regards as the most compelling evidence for induced mutation. He states that it is possible, by treating spring wheat for a few generations with the phasic conditions proper to winter wheat, to convert it into a winter wheat. Conversely, by treating winter wheats for a few generations with the phasic conditions proper to spring wheat, they may be converted into spring forms. Lysenko's[d,f,l] classical experiment along these lines, which is quoted extensively by Prezent and himself, is his work on Kooperatorka winter wheat. A sowing of this variety was made on 3 March in a temperature greenhouse, one plant of which survived and came into ear on 9 September. The grain from this plant was sown immediately in a warm greenhouse and this produced an F_2 generation which eared in the following January. Seed from this generation were sown under warm conditions on 28 March and produced a good crop in August, the grain of which behaved thereafter as a spring form. The whole process is described as high temperature vernalization, and since high temperature constitutes the "requirement" of spring cereals, the process is regarded as modifying the genetic constitution of the original winter wheat in the direction of the conditions applied.

Lysenko[q,r] devotes several other papers to the defence of this claim, admitting, however, that the new forms so obtained may revert if the phasic conditions are reversed. He quotes as another example the spring wheat Pallidum 032 from which a hardy winter wheat has been derived.

As another example of the effect of environmental conditions on genetical constitution, he mentions an experiment of Avakjan who crossed the two wheat varieties Hostianum 0237 and Erythrospermum 1160. The F_1 hybrid from this combination develops normally at first, but later the first-formed leaves wither so that at any given state in development only two healthy leaves are present. This dying back, Lysenko asserts, is attributed by Mendelian geneticists to the action of complementary lethal genes. He goes on to report that Avakjan found that by sowing Erythrospermum 1160, a spring wheat and the male parent of the cross, for two generations in the autumn, it could be crossed with Hostianum 0237 to give a normal F_1 hybrid exhibiting no lethal effects. Lysenko concludes that the explanation offered by geneticists involving lethal genes is erroneous, and that the hereditary nature of Erythrospermum 1160 has been modified by the effect of autumn sowing. In another experiment, Hostianum 0237, which is a winter wheat, was emasculated and pollinated with a mixture of pollen from the maternal variety and from Erythrospermum 1160. All the progeny were spring forms, so it was presumed that all were hybrid, yet the plants developed normally, a fact interpreted as further evidence against the theory of lethal gene action.

These data and others similar led Lysenko to the generalization that "the hereditary nature of any character can be changed in a controlled direction corresponding to the action of the external conditions applied",[1] a conclusion also affirmed by Kružilin and Keller.

The subsidiary evidence is quite copious and will be summarized shortly. Rubcov, in 1937, found that spring sowing of winter wheat and rye varieties altered their hereditary constitution. In 1939, Dolgušin[b] asserted that environmental factors have a significant effect on the hereditary constitution of wheat as regards the expression of grain yields. Šimanskii, next year, published details of the behaviour of the spring wheat Erythrospermum 1160 when winter sown. He obtained after several sowings a winter wheat that was more winter hardy, tillered better, was taller and had larger ears than the spring-sown controls. It was stated also that most of the winter sown plants survived, which indicated that the treatment was not merely a sifting out of a heterogeneous population. In this year also, Timofeeva-Tjulina reported changes in winter hardiness in wheat wrought through changes in the environment. Maljcev in 1941 claimed that seedlings of the wheat variety Milturum 0321, after treatment with a solution of sodium chloride, gave rise to strains tolerant of saline soils. Nazarenko, about the same time, investigated the effect of subjecting Ukrainka wheat to various cultural conditions, and claimed that hereditary differences in drought resistance had been induced, differences which persisted into the F_2 generation. Attempts to breed a wheat hardier than Lutescens 0329 were initiated by Lysenko[s] in 1942, the method being the cultivation of this variety under extremely severe conditions. Kononenko, at the same time, reported experiments with the three wheat varieties Hostianum 0237, Ukrainka and Krymka, which were each divided into four lots, and sown for three consecutive years in August, September, October and November respectively. The October-sown plants were declared to have become 20–25% more frost resistant than the typical varieties and to have exhibited differences in yielding capacity and vernalization response. Again, it was

[1] изменение наследственности любого свойства адекватно, соответственно воздействию условий внешней среды.

claimed that all plants survived the treatment, and therefore that the results were not to be explained as due to the sieving effect of late sowing. Šnaĭderman has published investigations on *Triticum-Agropyron* hybrids involving the combinations *T. vulgare* x *A. intermedium* Host, *T. vulgare* x *A. elongatum* Host, *T. durum* x *A. intermedium* and *T. durum* x *A. elongatum.* He found that the morphology and degree of sterility of the hybrids was affected more by the environmental conditions under which the crossings were made than by such characters as chromosome number. Somewhat similar observations had been made earlier by Lysenko,[h] who claimed that a self-sterile clone of *Agropyron* sp. became self-fertile during the process of vegetative multiplication.

Experiments with species other than cereals have also been made. Arnautov claims that hereditary differences in the yield, degree of earliness and starch content of potato varieties can be induced by modifying the fertility of the soil in which the tubers are grown. Lysenko[q] reports that summer sowing leads to an inherited improvement in the yield of potato plants. Several experiments with potatoes have also been made by Maksimovič.[a,b] He cut tubers of the variety Epicure in half and grew one lot of the halves in Rostov and the other in Moscow. On collecting the subsequent tuber generations and replanting both in Moscow, he found that the plants from the Rostov tubers yielded 20% less than the Moscow clones and showed a mosaic infection of 46% in contrast to 32% in the Moscow plants. He also grew F_1 seedlings obtained by selfing the variety Sinecvetka in peaty and in sandy soils respectively, and found that in the first case, dark blue tubers were produced, while in the second, the tubers were white. Gluščenko[a] investigated the effect of environmental conditions on potato clones and reported that the yields of tubers from clones grown in Moscow but derived from seed tubers obtained by summer planting in the south of Russia, were higher than the yields of the same clone that had been maintained in Moscow, and were higher than the yields derived from seed tubers derived from spring planting in the south.

Both in 1936 and 1937, Lysenko[h,l] alluded to experiments in which a self-sterile clone of beet, propagated by means of root crowns, became interfertile, especially when the combinations effected were between individuals grown under different environmental conditions. Analogous results were obtained by Kovalevskaja for carrots and radishes. In the case of the carrot experiments, it was claimed that if the roots were divided and then grown in the same pot, the seed yield obtained by interpollinating was 0.65 grm. If, however, the root segments were grown in separate pots and exposed to different conditions of light and temperature, the seed yield increased to 3.84 grm., and could be further increased to 5.84 grm. by planting the root segments in different soils.

Zemit investigated the effect of spacing on the hereditary constitution of flax, and found that the seed from closely spaced plants gave rise to less vigorous offspring and yielded less fibre and seed than seed obtained from widely spaced plants. Investigations of peas were conducted by Puhaljskaja, who reported that plants of the variety Irlandec grown in Leningrad from seed gathered from plants growing in four different Russian localities, differed significantly in flowering time and protein content, and for three consecutive years, in yield.

The great interclonal variation of the apple varieties of the Volga basin, especially of the Anis types, is attributed by Levošin to the effect of environmental factors, while Jablokov[a] found that F_1 poplar hybrids made out of doors differed genetically from those made by crossing cut branches in the laboratory. Finally Keller in 1945 reported that several plants which at first would barely tolerate the high sodium content of solonchak soils, would, when forcibly grown thereon, become eventually intolerant of any other.

These reports and various general discussions by such authors as Dolgušin,[a] Puhaljskiĭ, Keller and Eĭhfeljd constitute a comprehensive attempt to establish the actuality of environmentally induced variation, and it is necessary now to consider its value.

As might have been expected, one of the first reactions to Lysenko's theory was the charge of Lamarckism, which was raised at first by Meĭster,[a] Konstantinov *et al.* and Zebrak,[d] and later by Sax.[a] Since this charge, however, involves *a priori* theoretical assumptions on the nature of genetical mutability, it can be ignored in a discussion on factual evidence.

Much more to the point is the suggestion made by Meĭster[a] and Konstantinov *et al.* that many of the supposed genetical transformations are merely due to the effect of selection, the various treatments applied merely sifting out aberrant forms from an original heterozygous population. This criticism is much to the point with reference to Lysenko's original experiment with Kooperatorka winter wheat where only one F_1 plant survived. Many Russian crop varieties are known to be markedly heterogeneous, though it must be admitted that both Šimanskiĭ and Kononenko, in their experiments, claim that all or most of the plants treated

survived. In general, however, the possibility of selection having modified the composition of heterozygous populations cannot be excluded.

What is perhaps a more serious objection is consequent on the fact that adequate statistical treatment of the data has rarely been attempted. The full genetical potentialities of the initial experimental material and its range of variation would certainly need elucidating before the actuality of induced mutation could be asserted. It would also be necessary to study the behaviour of the progenies of the affected plants in order to exclude the possibilities that the induced change was merely temporary.

Meĭster's[a] suggestion that vernalization may induce mutation by bringing about chromosomal aberrations does not contradict Lysenko's thesis that induced changes do occur, although this mode of explanation would probably not appeal to Lysenko. Neither can Lysenko's claim of the actuality of induced mutation be weakened by Haldane's[b] insistence that such effects are extremely rare.

Haldane's objection, however, does tell against Lysenko's extended hypothesis that any character can be changed in the direction of the conditions applied. By over-generalizing in this fashion, Lysenko has gone far beyond his facts, and has proposed once again an elastic hypothesis incable of disproof, since he may always appeal to unknown factors when confronted with a character hitherto known to be invariable. Such an extension of his theory is worthless unless the type of conditions able to induce mutation can be specified. No attempt will be made to discuss this extension further.

Returning to the narrower theory that environmentally induced mutations do occur, it is extremely difficult, if not impossible, to draw a positive conclusion on the Russian evidence. It is possible, however, to examine the evidence recently put forward in England and America as to the actuality of directed mutation. The recent papers by Morgan, Haldane[c] and Emerson, although perhaps not completely convincing, at least place the problem on a more satisfactory basis and suggest the advisability of further research.

At the present moment, a decision for or against the reality of directed mutation seems premature. What is required is further experimental data produced under carefully controlled conditions, and then perhaps it will be possible to decide the matter as it should be decided, not by charges and rejoinders of Lamarckism and anti-Darwinism, but by unbiassed appeal to the facts.

Segregation

Inspired in part by a dislike of Mendelian explanations, Lysenko and his followers have applied themselves to investigating segregation phenomena believed to contradict Mendelian principles.

Long before the introduction of Lysenko's theories, Mičurin had suggested that the transmission of hereditary characters depended on the vegetative vigour of the parents, a point of view with which Krenke showed some sympathy. Mičurin also believed that the maternal parent tended to transmit its genetical characters more readily than the paternal.

Lysenko and Prezent adapted Mičurin's theory and extended its scope to apply to segregation in general. Lysenko[h] in 1936 and Prezent[e] in 1937 and 1939 insisted that the segregation ratios of hybrid plants depended on environmental factors. Prezent concludes that "it is only necessary to choose biologically significant conditions and you will get a different type, a different picture of segregation".[1]

The evidence is not very extensive and is derived largely from Titov's[a,b] investigations into wheat hybrids. He made a series of three crosses, Khorassanicum 01248 x Albidum, a red grained wheat x a white grained wheat, and a cross between two white grained varieties, and investigated the F_2 hybrid progenies in each of the years 1933, 1937 and 1938. Different segregation patterns were reported in the various years. In the first cross, all the F_2 plants were awnless in 1933, while 10.9% of awned plants appeared in 1937 and 11.1% in 1938; in the second cross, the ratios of red to white grained plants in the F_2 were 189 : 58, 122 : 50 and 6 : 6 in each of the three years respectively; while in the third cross, although white grained plants alone appeared in 1933 and 1938, three red grained plants were noted in 1937. Šulyndin[d] has written on "secondary segregation", which is the name given to a phenomenon reported by him at Smolensk, where supposedly fixed wheat varieties derived from the cross *Triticum durum* x *T. vulgare* began segregating after introduction from outside.

---

[1] Необходимо только подобрать биологически-значимые условия, и вы получите другой тип, другую картину расщепления.

One other instance of variable segregation has been reported by Kovarskii for the cross *Vigna sinensis* x *V. Catjang*. A larger proportion of grey seeds appeared in the F_2 when the F_1 was sown in spring than when it was sown in summer. Differences were also noted between the progenies derived from the spring and summer sowings in such characters as seed size, and shape and colour of the pods.

It is difficult, however, to accept the evidence provided above as an adequate basis for the theory of variable segregation. Once again the few experiments made, and the lack of statistical treatment, deprive the results of much value. It is not at all clear that the differences observed were not within the range to be expected from consideration of the laws of probability, and it is also possible that some of the differences noted might have been due to insufficiently rigorous experimental technique. Catcheside has recently analysed Zickler's data on the genetics of the ascomycete *Bombardia lunata* Zickler, and has shown that the segregation of two allelomorphic pairs did not occur at random but was polarized, the normal allelomorphs tending to pass to the spindle poles nearest the upper end of the ascus, while the mutant alleles tended downwards. Such evidence is interesting and suggests that Mendel's second law is in need of more careful formulation. The theoretical consequences of non-random segregation will be discussed later. Here it is only necessary to remark that the evidence offered by the Russian investigation of large deviations from Mendel's second law under field conditions is not yet convincing. Still less do the data provided demonstrate the action of environmental factors. It is possible, of course, to treat all differences in segregation ratios as resulting from the action of biologically significant conditions as Prezent has suggested, for, since the conditions are only known *ex hypothesi*, their mode of action can be postulated at will. But this would be merely another elastic hypothesis. As such it is not susceptible of logical refutation, but since, like the other elastic hypotheses mentioned, it can only be maintained by appealing to the unknown, its value is no more than that of the others.

Millardetism

The importance that Timirjazev attached to the work of the French botanist Millardet has already been noted. In his classification of the modes of inheritance, Timirjazev described as Millardetism those instances in which non-segregating progenies arose from F_1 hybrids. Lysenko and Prezent follow Timirjazev closely in this matter, and have even had Millardet's original articles translated into Russian and published in *Jarovizacija*.

It is to be expected that some of the offspring of a normal hybrid, those homozygous for all the dominant factors of the hybrid, will resemble the F_1 generation, but these will be accompanied by other segregates. Cases are known, too, of true-breeding hybrids as in *Oenothera* L. and other genera, where heterozygous forms are perpetuated by aberrant cytological mechanisms. None of these cases has much in common with the Russian examples, which are regarded as illustrating the permanent annihilation of the recessive factors of the hybrid.

The evidence provided by Russian geneticists is meagre. Lysenko's[m] original claims were based on observations made with Krymka wheat. He claimed that different forms of this variety, when intercrossed, sometimes gave rise to non-segregating hybrid progenies. More detailed investigations on this subject were made by Šulyndin,[e] also on wheat crosses. He reports that the offspring of a cross between *Triticum durum* var. *horanicum* and a Mediterranean form of the same species gave rise to nothing but *horanicum* types from the F_1 to F_6 inclusively. Similarly he notes that *T. dicoccum* is permanently dominant to *T. durum*. A proviso is made that dominance is not always complete and that recessive characters sometimes appear in the juvenile stages and disappear as maturity is approached. An anonymous[d] report in 1943 claimed that no segregation appeared in several intervarietal crosses of rye and wheat, even when the parental types were markedly dissimilar in morphological characters.

Lysenko remarks that he had formerly followed Mendelian geneticists in supposing that F_2 segregation always occurred after hybridization, but that he realized later that this assumption was incorrect. Invoking once again appropriate biological conditions, he maintains that it is possible to eliminate entirely F_2 segregation if only the correct conditions are applied. But again, the fact that the appropriate conditions are unknown in most cases militates against the value of his theory, and puts it on a par with the other elastic hypotheses already discussed.

As for the evidence upon which this generalization is based, it is obvious that this is at present insufficient for any satisfactory conclusion to be drawn. Unless a far more cogent series of observations can be produced demonstrating permanent dominance, it will hardly be possible to regard this theory as more than speculative.

F_1 Heterogeneity

According to Mendel's principles, crosses between dissimilar homozygous parents produce a uniform F_1 generation, variation in the progeny not appearing until the F_2 and subsequent generations. Lysenko and Prezent, in their attempt to disprove the theories of Mendelian genetics, focussed attention on this particular statement and claimed that exceptions to it occurred.

Detailed investigations on this topic are few. Šulyndin[b] reports that the F_1 generation of a cross between red and white grained wheats included offspring with red, white and intermediate grains. Differences in the F_1 progeny of crosses of *Cucurbita* sp. have been claimed by Krevčenko, while Jakovuk[a] has found differences in the F_1 generation of crosses between apparently homozygous tobacco varieties.

As they stand these results are suggestive, but unfortunately a rigorous attempt to make sure that the parents were homozygous does not appear to have been made. Without satisfactory evidence that the parents chosen were in fact homozygous, the results demonstrate little beyond the nature of the potential variability of the lines used.

Lysenko's theory of F_1 heterogeneity is a special case of his general theory of pure line variability. If, as this theory holds, two offspring of a homozygous individual may differ genetically from each other through the operation of appropriate environmental conditions, then two F_1 hybrid offspring of a cross may also be dissimilar. The evidence for this theory has already been considered so that it is unnecessary to go further into it here.

Reciprocal Hybridization

One of the characteristic features of the original Mendelian theory was its indifference to the direction of crossing. In so many cases is it known that the progeny of reciprocal crossings do not differ significantly, that the exceptions are ignored in formulations of the normal laws of heredity.

Yet exceptional instances in which reciprocal crossings differ from each other have long been known and were investigated in Russia by Mičurin, who thought that the maternal parent tended to transmit its characters more readily than the paternal. Later experiments to demonstrate reciprocal differences have been made by Šulyndin[a] for wheat.

Lysenko and Prezent have laid some emphasis on cases of reciprocal differences, which they seem to regard as contradicting Mendelian theory. This is true only when Mendelism is treated without reference to its development during the past three decades. Reciprocal differences, usually attributed to cytoplasmic factors, are admittedly not the same as the nuclear factors usually considered, but they can be and have been quite comfortably accommodated within the framework of present-day genetical theory.

Since so many cases of reciprocal differences in hybridization are known, it will not be necessary to cite the evidence here.

Internal Genetic Variation

One of the most original developments of Lysenko's genetical system is his contention that the various parts of a single individual plant may become genetically differentiated through the operation of biological conditions different in the various regions of the plant. His theory that the genetical nature of a plant is plastic and is alterable through the action of appropriate biological conditions has already been noted. The present theory is but an extension of this, and is a not unreasonable deduction from it. It is well known that the internal environment of plants is not uniform, nor are the different parts subject to the same external conditions; therefore, if the plant is able to respond adaptively to external conditions, a similar capacity might be expected of its parts.

Amongst the earlier investigators on this problem, those of Jablokov[a] are of special interest. He reports that in the cross *Juglans mandshurica* Maxim. x *J. Sieboldiana* Maxim., the progeny derived from any one branch of the female parent are similar to each other but differ significantly from the progenies of other branches in such characters as pubescence of the twigs, viscidity, maturation time and various floral and fruit characters. In the cross *Populus tremula* L. x *P. Bachofenii* Wierzb. var. *pyramidalis*, the progenies from different catkins are reported to differ significantly, while those from individual ovaries in a single catkin are claimed to be similar. Jablokov interprets these results as demonstrating that the parts of the maternal plants are genetically differentiated, the branches of *J. mandshurica* being slightly different *inter se* in their genetical constitution, also the different catkins of *P. tremula*.

Parallel observations have been made by Udoljskaja, who planted in separate lots the progenies derived from the upper, middle and lower parts of the ears of F_1 wheat hybrids. It was

discovered that more awned plants appeared in the offspring of the upper spikelets than in those of the lower. Muhin worked with barley, and found significant differences between the progenies derived from the upper, middle and lower regions of the ear, also between the first, second and third flowers of a single spikelet. In both cases, these findings were interpreted as evidencing heritable differentiation between the parts of the maternal parent.

Voronjuk in some interesting investigations on vetch, found that the plants produced from the lower pods differed from those derived from the upper in being earlier and larger, the F_2 plants being also larger but not earlier. He declined, however, to draw any definite conclusion from his results.

The notion that the dominance or recessiveness of a character could vary within a single individual was suggested by Jakovuk.[a] He reported that in certain F_1 tomato hybrids, derived from early sowings, the lower parts resembled one parent while the upper resembled the other. In later sowings the whole of the hybrid plants resembled the upper halves of the earlier hybrids.

General support for the notion of internal genetic variation is to be found in the writings of Lysenko[t], Gluščenko[d] and Keller, and the possibility of such a phenomenon is considered by Krenke. Lysenko mentions as additional examples instances of adventitious tuber buds in the potato giving rise to new forms, and also the prevalence of bud sports in fruit trees.

It seems that several different types of phenomena are being considered. Lysenko's examples of bud sports in the potato have been shown as due, in some cases at least, to the fact that many potato varieties are periclinal chimaeras. Bud sporting in fruit trees, on the other hand, is usually regarded as due to mutation.

There are probably no geneticists today who would deny that a single individual plant may comprise tissues of various genetical constitutions. Many instances of chimaeras are now known, and also many examples in which trees have produced bud mutations that have given rise to branches genetically different from those in the remainder of the tree. But these cases are somewhat exceptional and hardly comparable with the instances quoted earlier in which genetical variation in response to local conditions is claimed to occur in normal herbs and trees.

The evidence, although interesting, cannot be regarded as conclusive. A repetition of the experiments, with every precaution to exclude the effect of chance variation, would be necessary before such observations could be finally accepted. Uncertainty as to the statistical significance of these experiments is again a deterrent against accepting them at their face value. It is possible that some new form of variation has been discovered, but great caution is necessary before coming to any decision. Further experimentation is the only method of finally deciding upon this issue.

Mixed Inheritance

Timirjazev, who first introduced the term "mixed inheritance", derived the notion from the French botanist Naudin, who, in his theory of *hybridation disjointe,* believed that the paternal and maternal genetical substances were transmitted intact to the F_1 hybrid, and that in the hybrid, these two elements sometimes aggregated together to form a mosaic, perceptible to the eye, of paternal and maternal types of tissue. Timirjazev's examples included mainly what are now known to be chimaeras, or instances of variegation in which the F_1 hybrids exhibited a mosaic of the two parental colours.

Lysenko[t] took over Timirjazev's theory and quotes the experiments of Avakjan and Jastreb on tomato grafts as evidence. These will be considered in detail in the next section, and it is only necessary here to mention that, according to these two authors, tomato hybrids derived from combining red and yellow fruited parents, may bear on the same plant, red fruits, yellow fruits and variegated fruits. They report a similar intermixture of inflorescence types.

Turbin has investigated several cases in which mixed inheritance was suspected. In a cross between Bison, a round-fruited tomato, and Borghese, an elongate-fruited species, F_1 plants were produced which bore on a single plant a range of fruit forms intermediate between the two parental types. He also crossed a dioecious tricarpellary cucumber with a hermaphrodite pentacarpellary variety, and obtained F_1 plants bearing a mosaic of floral types intermediate between the parental extremes. And finally, in a cross between an entire-leaved Chinese pear and a central Asiatic variety with dissected leaves, he obtained F_1 plants bearing a mosaic of entire and dissected leaves.

Filippov[c] studied inheritance in the potato. He reports that when a plant of the variety 8760 (Blight Resister), a blue-tubered form, was pollinated by *Solanum andigenum* Juz. et Buk., a white-tubered form, the seedlings bore tubers of two kinds, white and particoloured blue and white. The particoloured tubers in the next tuber generation gave rise to plants bearing again

white and particoloured tubers, and so did the white tubers. The F_2 seed progeny of the F_1 seedlings gave rise also to plants bearing a mixture of white and particoloured tubers.

Upon these reports Lysenko has attempted to secure a solid foundation for his theory of mixed inheritance, a matter requiring careful examination.

In the first place the reality of mixed inheritance is well established in chimerical plants, such as certain potato varieties, which may be propagated vegetatively by means of tubers. The instances of variegation quoted, however, cannot be accepted without question, for it must not be assumed that a phenotypic mosaic pattern implies a corresponding mosaic of genetically differentiated tissues. Many of the instances so far investigated of variegation have been shown to arise in genetically uniform plants by the differential development of pigment during the course of development. The various types of mosaic mentioned above may therefore be due, not to a mosaic of genetically different tissues, but to the operation of the various ontogenetical factors which bring about tissue differentiation during the course of development. In the examples quoted it would also be useful to know the range of variation of the parental types and their behaviour under various environmental conditions. Only after such preliminary investigations could the significance of the F_1 variation be elucidated.

Perhaps the best evidence for the existence of mixed inheritance in sexually reproducing plants is to be found in cases involving chlorophyll variegation caused by cytoplasmic factors. It has been found by Wagner that pollinating a normal rye plant with pollen from variegated plants, and by repeatedly back-crossing the progeny to the variegated forms, using the latter as the male parents, patches of variegation ultimately appear in the later generations. This is explained by assuming that variegation is controlled by cytoplasmic factors which accumulate little by little in each back-cross, until they reach a threshold at which they can bring about observable effects.

Timirjazev's notion of mixed inheritance is a composite idea, which has been resolved by later research into at least three different phenomena, or four if the variegation due to viruses is included, and can hardly be applied usefully today, without relating the instances cited to more recent work. None of the well-authenticated cases of mixed inheritance described appear to be outside the realm of modern genetical theory.

Graft Hybridization

The controversy as to whether or not graft hybridization occurs is one of the several long-standing problems of biology. In the sense that Darwin[b] understood the expression, graft hybridization meant the transmission of the characters of two parents to a single offspring, not by normal fertilization, but by means of the fusion of somatic tissue that occurs during grafting. Most of the classical instances of graft hybrids have since been shown to be chimaeras, but there are still several obscurities concerning the nature of some of them. Weiss in 1930, and Neilson Jones, a few years later, have reviewed the various problems relating to graft hybridization, and incline to the opinion that hereditary interaction between the stock and scion of grafts is not yet demonstrated.

In Timirjazev's time the nature of graft hybrids was still problematical, and he accepted Darwin's opinion that genetic interaction between stock and scion was possible.

It is necessary in this connexion to refer to Mičurin's theory of vegetative rapprochement,[1] which, together with Timirjazev's Darwinian notion of graft hybridization, formed the antecedent for Lysenko's theory. At the beginning of the century, Mičurin[b] devised his mentor method of vegetative rapprochement in order to reduce the sterility often existing between varieties or species of fruit trees which were being hybridized. In this technique, the mother tree was used as a stock and a branch of the paternal parent, the mentor, was grafted upon it. Then, according to Mičurin, the genetical natures of the two elements of the graft should approach each other with an accompanying decrease in intersterility. The important point to notice is the assumption that the mere action of grafting is sufficient to bring about hereditary changes. Later Mičurin used the term "mentor" for any scion that was used for effecting hereditary changes in a stock or even for a stock that was used to effect changes in a scion. Mičurin's method of vegetative rapprochement, which began as a practical technique for reducing intersterility, ended as a method for inducing hereditary changes. A series of papers defending its validity appeared from 1934 to 1936 by such authors as Tatarincev and Silantjev, Odincov, Gorškov and Jakovlev, Gurov and Sludskii. Tatarincev[b] performed experiments on vegetative rapprochement between the pear and *Sorbus Aucuparia* L. and claimed to have obtained an

---

[1] вегетативное сближение.

increase in the number of fruits reaching maturity, also higher fruit and seed weights. Sasonkina asserted that vegetative rapprochement was effective in reducing intergeneric sterility between *Lupinus angustifolius* L., *L. polyphyllus* Lindl., *Pisum sativum* L., *P. arvense* L., *Lens* sp., *Phaseolus* sp. and *Vicia* sp., but these extraordinary claims have not been repeated. Rather later, in 1940, Venjjaminov has suggested that mentor effects may be due to acceleration in mutation rate.

It is particularly important to study the literature on graft hybridization published by Lysenko and his school, because, after 1940, evidence on the anomalous effects of grafting has come to constitute the principal experimental basis for Lysenko's theories. It would seem that the various lines of evidence already mentioned had been felt as not altogether adequate for the establishment of a new genetical system, and consequently, after this date, an increasing emphasis is placed on grafting data.

One of the principal papers on this subject, which is referred to in Lysenko's[t] general exposition of his theory, is by Avakjan and Jastreb, and was published in *Jarovizacija* in 1941. It deals principally with tomato grafts and will be summarized in some detail, since it is one of the corner-stones of Lysenko's system.

The authors investigated the effect of grafting the tomato variety Albino, a form with compound inflorescences and large, multilocular pale yellow fruits, on Mexican 353, a variety with simple inflorescences and small, bilocular red fruits. They found that after grafting, the scion bore fruits of various colours: red, pink, yellow, and yellow with pink stripes. These fruits when planted gave rise to F_1 plants bearing fruit of the following colours: red, bright pink, pale pink, golden yellow, pale yellow, and pale yellow with pink stripes. In the F_2 the fruit colorations observed were red, bright pink, pale pink, orange, golden yellow, pale yellow and pale yellow with pink stripes. The results are shown in Table 1, where, however, the yellow fruits

TABLE 1

PROGENIES OF SELECTED FRUITS OF ALBINO GRAFTED ON TO MEXICAN 353

		Number and colour of fruits in the progenies of the selected fruits						
		Red	Bright pink	Pale pink	Orange	Golden yellow	Pale yellow	Pale yellow with pink stripes
Progenies from selected fruits of the scion classified according to colour	Red	3	4	–	–	2	1	–
	,,	3	2	–	–	1	1	–
	,,	29	9	–	–	9	7	–
	Bright pink	5	1	–	–	1	2	–
	,,	2	1	–	–	3	1	–
	Pale yellow with pink stripes	–	–	–	–	–	9	5
		–	–	–	–	–	5	4
	,,	3	–	–	–	–	2	33
	,,	74	–	–	–	–	3	–
	,,	1	–	5	–	–	8	136
Progenies from selected F_1 fruits classified according to colour	Red	23	–	–	7	–	–	–
	,,	9	11	1	5	1	–	–
	,,	20	8	–	1	1	–	–
	,,	7	4	1	1	1	–	–
	Bright pink	–	44	–	–	–	–	6
	,,	–	29	–	–	–	–	5
	,,	–	14	–	–	–	–	2
	,,	–	11	–	–	–	–	4
	,,	–	29	–	–	–	–	7
	,,	–	28	–	–	–	–	6
	Golden	–	–	1	8	13	3	4
	,,	–	–	2	12	4	6	6
	,,	–	–	2	2	2	–	–
	,,	–	–	7	2	11	1	3

of the scion mentioned in the text are not included, an unfortunate omission since the progenies of these fruits would make interesting controls. The distinction between golden yellow and pale yellow fruits is based on the fact that in the former the fruit skin is pigmented.

It is to be regretted that these interesting data are not accompanied by data for the controls, especially as several tomato varieties are known to be far from homozygous, and it is consequently very difficult to determine how far the data provided denote a possible heritable effect by the stock on the scion, or how far they merely represent the natural variation of the variety Albino. The grouping of the data also suggests that personal bias might possibly be significant. It is noteworthy that few pale pink and no orange fruits occur in the F_1. In the F_2, all the progeny of the bright pink F_1 fruits are either bright pink or pale yellow with pink stripes, yet the red F_1 fruits produce a range of F_2 fruit colorations, including red, bright pink, pale pink, orange and golden yellow, and the golden F_1 fruits give rise to fruits either pale pink, orange, golden yellow, pale yellow or pale yellow with pink stripes. An appropriate means of estimating any possible effect due to personal bias in grouping the data would have enhanced the value of the results considerably.

Corresponding data in this same cross for inflorescence type, whether simple or compound, are presented in Table 2.

TABLE 2

PROGENIES OF SELECTED FRUITS OF ALBINO GRAFTED ON TO MEXICAN 353

	Number of F_1 plants classified according to inflorescence type		
	Simple	Compound	Simple and compound on same plant
Progenies from	3	34	—
selected fruits	24	14	39
of the scion	2	54	2

Here again, no control data are presented.

Avakjan and Jastreb also investigated the genetics of several other tomato grafts. When Mexican 353 was grafted upon Albino, the reciprocal graft of that already mentioned, they found that the stock, Albino, produced offspring bearing red, pink and golden yellow fruits, the F_2 of the red fruits including forms bearing red, pink, golden yellow and pale yellow fruits, the F_2 of pink fruits being pink and yellow fruited, while the golden yellow F_1 fruits gave rise to orange and golden yellow fruited progenies. A graft of Albino upon a potato-leaved variety with simple inflorescences and small red bilocular fruits was also studied, and the F_1 from the fruits of the scion were found to include forms with simple inflorescences, with bilocular or trilocular ovaries, and with reddish orange fruits, some spotted with yellow. When Albino was grafted upon Preserving, a dwarf variety with clustered leaves, simple inflorescences and small red bilocular fruits, the fruits of the scion gave rise to F_1 plants bearing simple inflorescences, small red and sometimes bilocular fruits; plants with dwarf habit and clustered leaves also appeared, and in some plants simple and compound inflorescences appeared on the same individual. A final graft of Albino upon New Tree, a variety with bright pink fruits, was studied, and in this the fruits of the scion gave rise to F_1 plants, some of which bore yellowish white fruits with a few pinkish stripes; a few large red fruits were also observed.

Grafts involving tomato varieties other than Albino were made as well. When the variety Best of All was grafted on Yellow Peach, a variety with yellow bilocular pubescent fruits, the F_1 generation from the fruits of the stock was found to include plants with various inflorescence types, and bore fruits of various sizes and shapes, with the number of locules ranging from two to nine, and including forms with reddish orange fruits and fruits yellow in colour with pink patches. Yellow Peach, when grafted on the variety Market Wonder, bore fruits which gave rise in the F_1 generation to plants with leaves less dissected than in the scion and to plants with red fruits. Bilocular fruits were found in the progeny of the scion in the graft combination Best of All on Yellow Peach. The results are shown in Table 3.

TABLE 3

PROGENIES OF SELECTED FRUITS OF BEST OF ALL GRAFTED ON TO YELLOW PEACH

	Number of F_1 plants classified according to number of locules		
	Multilocular	Trilocular	Bilocular
Progenies from	4	7	1
selected fruits	9	—	—
of the scion	9	14	3
	—	11	2
	10	—	—

Lastly, a graft was made of *Lycopersicon pimpinellifolium* Mill. var. K-1014, a variety with simple inflorescences and small bilocular or trilocular fruits, on Rosso Grosso, a variety with compound inflorescences and large multilocular fruits. The characters of the F_1 progeny from the scion are given in Table 4.

TABLE 4

PROGENIES OF SELECTED FRUITS OF *L. pimpinellifolium* K-1014 GRAFTED ON ROSSO GROSSO

	Number of F_1 plants							
	Classified according to inflorescence type				Classified according to number of locules			
	Simple	Compound	Simple and compound types on same plant	Total number of plants	Bilocular and trilocular	Multi-locular	Both types on same plant	Total number of plants
Progenies from selected fruits of the scion	5	—	—	5	—	4	1	5
	13	—	—	13	1	2	10	13
	25	39	9	73	3	39	5	47
	12	23	2	37	1	28	4	33
	4	24	1	29	—	24	—	24
	6	21	5	32	—	20	1	21
	11	2	4	17	9	5	3	17
	15	3	—	18	—	11	4	15
	20	6	—	26	—	12	8	20
	—	9	—	9	—	9	—	9
	18	5	1	24	5	4	3	12
	1	26	2	29	2	32	2	36

In this table, the F_1 progenies have been classified in two ways, according to inflorescence type and according to the number of locules in the fruits. It is rather odd to discover that the total number of plants in the progenies from each fruit (not calculated in the original tables) are not always the same in the two classifications. In fact only in four cases do the totals tally. In most cases the total number of plants is less when these are classified according to inflorescence type, than when classified according to locule number, but in one case, the opposite is observed. It might be supposed that the discrepancies are due to the fact that some of the inflorescences did not develop sufficiently to enable an examination of the number of locules to be made, but this would hardly account for the very large discrepancy in the third line, nor for the fact that in one case (the last line of the table) more plants apparently bore fruits than produced inflorescences. These puzzling features make it difficult to accept the figures in this table without reserve, and rather prejudice the reader against accepting the other data of these authors without careful analysis.

It has already been mentioned that Lysenko regards the work of Avakjan and Jastreb as furnishing one of the most important pieces of evidence for his genetical theories, and for this reason their data have been considered in some detail. It cannot be denied that their results are somewhat unexpected as they stand, so that it is very important that their validity should be beyond question. Unfortunately, the lack of data relating to control experiments and the doubt about some of the tabular information makes it impossible to accept them without confirmation. It would also be desirable to know more about the technical details of experimentation. Lysenko mentions that, in making these grafts, the scion should be as young as possible and the stock of medium age, and that, when the graft has taken, the leaves should be removed from the scion but left intact on the stock. He added in 1943 that it is advisable to bag the flowers in gauze in order to eliminate the danger of occasional cross-pollination. Avakjan and Jastreb however make no mention of precautions against chance cross-pollination, and it seems that Lysenko's reference to the advisability of bagging was only made after criticisms by his opponents of his experimental technique. Stray cross-pollination would certainly explain many of the results of Avakjan and Jastreb, and until this and the other points militating against their results can be removed by confirmatory experiment, it is only possible to suspend judgment in the meanwhile.

There are also other Russian experiments on tomato grafts which must be mentioned. Boldyrev grafted tomatoes on potato stocks and claimed to have obtained from the fruits of the scion, F_1 plants that differed from the controls in such characters as colour, degree of pubescence, leaf form, flowering time and fruit shape. A year later, in 1941, Solovjeva reported

that the tomato scions of the bilocular variety Humbert, when grafted on to *Nicotiana Tabacum* L., gave rise to F_1 progenies with small amounts of nicotine or some allied substance. The same tomato variety was grafted by Hazina on *Solanum nigrum* L. and on *Lycium barbarum* L. In the first case, the progenies from the scions were reported to be more vigorous than the controls and these on regrafting on to *S. nigrum* were found to give rise to progenies exhibiting variation in fruit shape and number of locules. The F_1 and F_2 progenies of the scions grafted on to *L. barbarum* included some trilocular and quadrilocular fruits. Ermolaeva[b] grafted tomatoes on to *Solanum demissum* Lindl., *S. acaule* Bitt. and *S. nigrum*, and found that the F_1 plants from the fruits of the scion exhibited variation in fruit size, Potašnikova, who grafted the tomato varieties Bison, Sparks Gribovo and Best of All on *S. nigrum*, claimed that the F_1 progenies of the scions exhibited increased resistance to cold, were 10–12 days earlier and bore larger fruits than the controls. These results and others were reviewed by Lysenko[t] in 1943, who affirms that grafting can bring about a heritable transmission from stock to scion of such characters as fruit colour, fruit size, locule number, plant habit, leaf shape, and length of vegetation period.

Three further papers on tomato grafts were published in 1944. Gluščenko[d] grafted the varieties Golden Queen on Ficarazzi, Golden Queen on Mexican 353, Golden Queen on Humbert and Planovyĭ on Gruševidnyĭ. He claimed as a result of his experiments that heritable differences occurred in the first seed generation of the scions in such characters as fruit colour and locule number. Tušnjakova grafted tomato on *Nicotiana rustica* and reported the presence of nicotine in the fruits of the three succeeding generations of the scion, the contents being 0.73%, 0.3% and 0.2%, respectively. By grafting *Datura* sp. on tomato, the converse effect was observed, and the F_1 *Datura* seedlings from the scion had a reduced atropine content. Grafts of the tomato variety Bison on potato gave rise to F_1 seedlings bearing tubers on their roots, and these gave rise in the next generation to three anomalous plants with numerous aerial and subterranean tubers. These anomalous plants could be propagated vegetatively by means of either the aerial or subterranean tubers. Gaškova grafted the tomato on the egg-plant and reported yield increases in the following seed generation of the scion, together with a reduction in the number of seeds. The number of locules in the F_1 was variable.

Work has also been done on potato grafts, especially by Filippov.[a] He was interested at first in vegetation period and reported in 1938 that grafts of the varieties Early Rose, Epicure and Lorch on either *Solanum demissum* or *S. acaule* led to the production of tubers in 60–70 days after planting out, the controls producing none after 200 days. These results were interpreted as confirming Mičurin's theory of vegetative rapprochement.

Shortly afterwards, Filippov[b] reported the results of an extensive series of grafting experiments involving the varieties Early Rose, Wohltmann, Epicure, Blight Resister, *Solanum acaule* and *S. demissum*. He found that when Early Rose was grafted on Wohltmann, the plants of the next tuber generation had certain resemblances to Early Rose in such characters as leaf form, tuber shape and tuber colour. In the reciprocal graft, the next tuber generation showed resemblances to Wohltmann in various haulm and tuber characters. Comparable results were obtained for grafts of Wohltmann on Epicure and Lorch on Early Rose. When Early Rose was grafted on *S. acaule* cuttings taken from the scion developed into plants with resemblances to *S. acaule*, while cuttings derived from the scion of the combination *S. acaule* on Wohltmann produced small tubers. Experiments were also reported to show the usefulness of the mentor method in securing seed sets from cross-pollinations between *S. acaule* and the variety Fürstenkrone, and between Lorch and *S. demissum*. These combinations proved sterile in the controls. Further experiments on the successful use of the mentor method were reported by Filippov[d] in 1941.

Other geneticists who have studied potato grafts include Maksimovič,[b] who grafted Fürstenkrone on Wohltmann and obtained an F_1 tuber generation which produced white tubers differing in shape from the typical Wohltmann type. Solodovnikov made grafts of Fürstenkrone on Wohltmann and found that the tubers produced by the F_1 and F_2 tuber generations had resemblances in shape to those of Fürstenkrone; they also had 3–5% less starch than the controls and were 25–30 days earlier. An F_1 tuber generation resistant to *Phytophthora infestans* (Mont.) de Bary was reported from a graft of *Solanum demissum* on Wohltmann, and similarly for a graft of *S. demissum* on Alma. Heritable changes in the tuber progenies were reported by Razumov[b] for grafts involving potato varieties, *S. Antipoviczii* Buk. and *Datura* sp.

Avakjan and Jastreb, whose work on tomato grafts has already been discussed, also made some potato grafts, and found that, when the variety Odenwälder Blaue was grafted on Ella, the F_1, F_2 and F_3 tuber generations were characterized by the blue pigmentation distinguishing the scion. Berljand grafted potatoes on tomatoes and found that the scions produced a few

aerial tubers, which, when planted, gave rise to F_1 plants also capable of producing aerial tubers. Grafts of *Atropa Belladonna* L. on potato were made by Tušnjakova, who claimed that the F_1 seedlings of the scion contained less atropine than the controls. Conversely, potato grafted on *Datura inermis* Jacq. produced aerial tubers which gave rise to F_1 plants containing 0.15% atropine in the leaves, stems and tubers.

Grafting experiments with *Helianthus* spp. have been conducted by Ananjeva.[a,b,c] The F_1 generation of *H. occidentalis* Riddell grafted on *H. annuus* L. was found to differ from the controls, while intervarietal grafts of *H. annuus* frequently gave rise to progenies more vigorous and earlier than the controls, and these had broader inflorescences and higher seed yields. Grafting on the dwarf stock Karlik proved deleterious. Ščerbina, who investigated the progeny of grafts of artichokes on *H. annuus*, found no heritable modifications.

Fomin grafted lentils on peas and declared that the F_1 progeny of the scion differed from the controls in various vegetative and seed characters. Wheat, which is not a very suitable subject for grafting experiments, has been investigated by Plotnikov. He found that when Lutescens 062, a variety of *Triticum vulgare*, was grafted on *T. durum*, it gave rise to F_1 plants with grains showing some similarity to those of *T. durum*; they were more vitreous and a darker red than those of Lutescens 062. A graft was also made of the wheat variety Candicans 75/09 on a wheat-rye hybrid 434/154. In this case, it was reported that the F_1 plants were intermediate between the stock and scion in such characters as leaf pubescence, grain colour and vitreousness. The production of a vegetative hybrid between wheat and *Elymus* sp. was claimed by Pisarev and Vinogradova using embryo grafts. Kopeljkievskiĭ and Mihaĭlov have experimented with buckwheat. When common buckwheat was grafted on to *Fagopyrum tataricum* Gaertn. or *Polygonum Weyrichii* Schmidt, the F_1 offspring of the scion were found to be more vigorous than the controls, to produce usually a higher seed yield, and to combine some of the morphological characters of both stock and scion. Increased seed size was reported for the F_1 offspring of French buckwheat grafted on the Japanese buckwheat. In other grafts, heritable differences were reported in the colour and morphology of the F_1 seeds.

One final set of observations needs mention and those are the researches on interspecific hybridization in *Populus* L. by Jablokov.[a] His experiments in support of Lysenko's theory of genetical variation within the individual have already been noted. Jablokov reported that F_1 interspecific hybrids of *Populus*, grown on their own roots, were genetically distinct from those grafted on to *P. nigra* L. var. *pyramidalis*. In 1945 a general account of genetic interaction between stock and scion and its implication for genetical theory was published by Šmuk.

The principal evidence that has been put forward by Lysenko and his adherents in favour of the theory that graft hybridization occurs has now been mentioned, and it will have become clear that the task of evaluating it is not easy. Žebrak[c] has recorded experiments in which grafts of the two pea varieties Thomas Laxton on Yellow Victoria, and grafts of male hemp on female, exhibited no genetical interaction, but Lysenko only claims that interaction occurs under appropriate biological conditions.

Haldane[b,c] in 1940 suggested that Lysenko's results might be explicable through the invasion of either stock or scion by a non-pathological virus from the opposite member of the graft. Later, in 1944, he suggested that Darlington's remarks on bud-grafting in roses, which causes in some cases a reversion of the climbing habit to the bush habit, might lend some support to Lysenko's theory.

The theoretical interpretation of the facts will be postponed until the next chapter. The point that must be considered here is whether or not grafting does induce heritable modifications. In the reviews on graft hybridization and chimaeras published by Weiss and Neilson Jones the possibility of such an effect is considered but regarded as undemonstrated. It is admitted, however, that if the extensive series of experiments of the French botanist Daniel[a,b,c,d,e] were confirmed, this opinion would need to be revised. Daniel has made a large number of grafts, principally involving fruit trees or *Helianthus* spp., in which he claims that heritable modifications have been obtained. So far his work has not been confirmed, so that it is impossible to regard these findings as established.

There is a further difficulty attending the interpretation of data relating to this subject, and this is the fact that no real distinction can be drawn between cytoplasmic inheritance and virus infection. It is well known that most viruses can be transferred from stock to scion by grafting, and, if such viruses proved seed-transmissable (and even this is irrelevant in vegetatively reproduced plants such as the potato) it would not be possible to distinguish such infection from an occurrence of vegetative hybridization involving cytoplasmic factors.

It is also difficult to distinguish between vegetative hybridization and the induction of

mutation in one member of a graft by the other. In either case the manifestations of the two phenomena would be similar and would need very careful experimental work to decide whether some element of one member of the graft had been transferred to the other or whether modification had occurred in some other way.

These problems, together with the lack of confirmation of most of the significant data and doubts as to its statistical significance, render the task of interpretation extremely difficult. The urgent need is for a repetition of some of Lysenko's experiments under strictly controlled conditions. Till this is done, no satisfactory conclusion can be drawn as to the significance of his data, and for the present, no other course is open beyond that of suspended judgment.

Summary

1. The genetics of earliness

Lysenko's theory that a hybrid F_1 plant is never later than the early parent is untrue, also his theory that transgressive segregation for earliness in the F_2 and later generations is impossible. When qualified, however, to apply only under appropriate conditions of existence, it cannot be disproved since these conditions are in most cases unknown. The theory is thus elastic and can be applied to all instances irrespective of whether it is true or false. Such elastic hypotheses are practically useless for scientific purposes.

2. The prediction of dominance

The theory that those characters are dominant which are best adapted to the environment of the hybrid has many exceptions. If, however, the environment is restricted to some unknown and early stage in the development of the seedling, the generalization becomes elastic and practically incapable of disproof. Thus formulated it is again practically useless.

3. Degeneration of pure lines

An inevitable deterioration in the vigour of pure lines has not been proved.

4. Rejuvenation

The evidence for the claim that intravarietal crossing is frequently beneficial is satisfactory.

5. Induced mutation

Although not yet adequately established, there is some evidence that environmental factors may induce hereditary changes more or less directly. Much of Lysenko's evidence is of little value.

6. Segregation

Certain instances of non-random segregation are known. Lysenko's data on the subject are not convincing.

7. Millardetism

Non-segregating hybrids are known in some cases. The Russian examples are not compelling.

8. F_1 heterogeneity

Heterogeneity in the F_1 generation of a cross between homozygotes may occur after mutation. Lysenko's data are susceptible of normal genetical explanation.

9. Reciprocal hybridization

Many instances of differences between reciprocal crosses are known.

10. Internal genetic variation

Chimaeras and plants bearing bud mutants are genetically diverse individuals. The theory that normal plants become genetically heterogeneous through the operation of environmental factors lacks cogent demonstration.

11. Mixed inheritance

Variegation may be due either to chimaera formation, differentiation of pigmentation during ontogeny, cytoplasmic factors or virus infection. There is no advantage in maintaining Timirjazev's category of mixed inheritance.

12. Graft hybridization

The evidence for genetic interaction between stock and scion is not compelling but suggestive. Further experiments are needed before a conclusion can be reached.

V. INTERPRETATION

Most of the evidence utilized by Lysenko in the elaboration of his genetical system has now been considered. It has been shown that some of this is not compelling, owing to lack of stringency in experimental technique. Other aspects, such as the work on intravarietal crossing, seem to be fairly well authenticated. Doubts as to the value of some of the data make a discussion of the theoretical conclusions drawn from them difficult. But it is possible to make some progress in this respect, and to analyse the validity of the generalizations that Lysenko and Prezent have made, merely in the light of their coherence with the presumed facts.

The theoretical assumptions of Lysenko and Prezent will be examined first, then the theories they have put forward to explain genetical facts in general and their own results in particular. But before going on to the general account of Lysenko's theory, it has been found necessary to give a brief consideration to the scientific value of its philosophical foundations. The theory is avowedly based on dialectical materialism and is so closely wedded to it that it is quite impossible to consider the one without the other. This is particularly necessary before examining Lysenko's criticisms of Mendelian genetics.

The relationship between dialectical materialism and biology is a difficult subject to elucidate and in a treatment of this question it is not always possible to avoid certain controversial issues. However, it must be pointed out that dialectical materialism is a philosophy admitting a diversity of interpretations. There are many geneticists in sympathy with this philosophy, who draw from it quite different implications than do Lysenko and Prezent. In this bulletin, it is the interpretation of dialectical materialism advanced by these two authors that is being primarily considered. Other exponents of this philosophy, both within and outside the Soviet Union, express philosophic views markedly dissimilar to those of Lysenko and Prezent. Such views will receive only incidental attention in this chapter since the examination of the relation between biology and dialectical materialism in general is another and very complex problem.

Dialectical Materialism

It has been accepted as a convention among western scientists that philosophy and science are disjunct branches of study and may well proceed independently one of the other. The more or less homogeneous conceptual framework of science, which has been generally accepted by the large majority of investigators, has been contrasted with the extreme divergence between the schools of philosophy. And the conclusion has been drawn that scientific conclusions may be derived without reference to philosophical questions.

In so far as the unity of science depends on the fact that its primary data are more or less the same for everybody, and require nothing more than a normal sensory system for their perception, this conclusion is justified. But when the transition is made from perception of sense data to generalization and hypothesis, the unanimity with which scientific concepts are held rests upon other bases. Of these, one of the most important is the acceptance, often unconscious, by scientists of a common philosophy or at least the acceptance of one of a group of philosophies that agree in their relation to scientific data.

In the west, most scientists have adhered, implicitly or explicitly, either to some form of realism—exemplified today by Whitehead's philosophy of organism—or to some form of materialism. The sceptical tendency in philosophy initiated by Hume and Kant and the post-Kantian idealistic philosophies have appealed much less to scientific investigators, though the *Naturphilosophie* of Schelling had a profound effect on morphological theories in the nineteenth century, and its effects persist still. The attachment of scientists to realism or materialism is sufficient to account for much of the unanimity with which scientific theories have been held. It also explains why the opinion has arisen that science is independent of philosophy. Unreflective scientists have failed to realize that realism or materialism are only two of many possible philosophies. The failure to analyse the philosophical assumptions involved in scientific research has encouraged a tendency to regard science as independent of philosophy, whereas in fact it becomes devoid of real significance unless grounded in philosophy. The meaninglessness of scientific conclusions if divorced from philosophy has been realized of late. It has resulted in one of two tendencies, either a return to philosophical appreciation by scientists, or a rather despairing conclusion that science is nothing but the inevitable consequence of the manner in which the human mind must relate its ideas, and is thus without real significance, an attitude common amongst the logical positivists.

In the main, then, scientists have adopted either a form of realism or materialism, often

without realizing that they were philosophizing, and have interpreted their data in accordance with these philosophies. It is unnecessary here to consider the divergencies between these two philosophies for the important point is that both concur in conceding the real existence of extra-mental things and in regarding scientific conclusions as relating to the nature of the real world.

The necessary dependence of science on philosophic presuppositions has been clearly realized in the Soviet Union. Here the philosophical ideas utilized by western scientists have been replaced by the philosophy of dialectical materialism founded by Marx and developed in particular by Engels[a,b] and Lenin. Both Lysenko and Prezent claim that their genetical theories accord with dialectical materialism. These they contrast with Mendelian genetics, which they claim to be inconsistent with this philosophy. No adequate examination of Lysenko's genetical theories can therefore be made until the truth value of his interpretation of dialectical materialism has first been considered.

With the sociological theories of Marxism, this bulletin has no concern. It is only necessary here to examine the validity of dialectical materialism as a pure philosophy, especially in its relation to biology. But it is not easy to obtain a clear picture of its tenets, since its various exponents are usually more concerned with sociological issues than with presenting a clear exposition of philosophical or scientific principles. The following account is drawn principally from the writings of Marx, Engels,[a,b] Lenin, Bernal,[a,b] Levy,Prenant, Haldane[a] and Prezent.[d,e,f]

The origins of dialectical materialism are fairly clear. Marx took over the vulgar materialism of the eighteenth century French materialists and the nineteenth century German philosophers such as Feuerbach, and combined with it a modification of the dialectical elements in the philosophy of Hegel. The notion of the dialectic goes back to Plato, who in his dialogues illustrates the development of ideas via the instrumentality of discussion, argument and counter-argument. Hegel adopted the Platonic notion and extended it to history, interpreting significant historical movements as the expression of a transcendental dialectic, which for Hegel was the evolution of the Absolute Idea. Marx took over the notion of dialectic as modified by Hegel, and declared that it represented a property, not of the Absolute Idea, but of matter. Dialectics for Marx is thus the study of those properties of matter which he attributed to it when he took over Hegel's notion. The essential philosophical tenets of dialectical materialism may be summarized shortly as follows:—

(1) Everything that exists is material.
(2) Matter is eternal.
(3) Matter is always changing.
(4) Matter comprises opposing elements whose interaction is the cause of change.
(5) Material change is historical.

The first tenet, that everything is material, is usually regarded by Marxists as self evident, or else examples are quoted to show how psychic states are conditioned by material factors. The Marxian analysis of mind can be ignored here as irrelevant, though it is important to note that Marx's notion of mind as reflecting matter, or as a function of matter, hardly elucidates the problem of what mind is. It is not clear either what precise significance is to be attached to the term "matter" itself, though Lenin seems to understand by it sense-perceptible being, a definition which is difficult to apply to many of the entities recognized by modern physics. The *a priori* denial of immaterial existence is an unsatisfactory element in dialectical materialism, since, although it may be held that the existence of material things is alone demonstrable, this belief derives from empirical observation and not from logical necessity. Philosophy should cover all possible forms of being, and as the notion of immateriality has never been shown to involve contradiction, it should not be denied *a priori*. By limiting his philosophy to observable entities and making the unproved assumption that everything that exists is sense perceptible, it can be maintained that Marx has forsaken philosophy for science, and introduced an anthropocentric limitation into philosophy, which should study being in general whatever its mode.

Belief in the eternity of matter is a corollary of the belief that nothing but matter exists. It has no important bearing on biology, although it seems to prejudge certain cosmological questions relating to the origin of the universe.

The next tenet, that matter is always changing, needs more consideration. Denial of the reality of change, as by Parmenides, is a point of view adopted by very few philosophers. On the other hand, the assertion of Heraclitus, which is quoted appreciatively by Marxists, that change is the nature of things, is similarly difficult to maintain. For most people, change involves a subject which changes, that is, an enduring substrate. If this is denied, it is hardly

logical to speak of change at all, but only of a succession of different states. In the interpretation of dialectical materialism made by Lysenko and Prezent, the duration of relatively unchanging elements such as the genotype is stigmatized as undialectical. Yet these authors seem to have some notion of individual duration, and with other Marxists are presumably committed to a belief in the duration of matter. There does appear to be some inconsistency in asserting that nothing remains unchanged in any respect, and yet continuing to assume that many things endure.

It is true that most material objects change continually in certain respects, if only in their spatial relations. But change in some of the properties of a thing by no means excludes the possibility that other characteristics remain unaltered. In fact, change as properly understood, in contrast to a mere succession of disconnected states, implies the persistence of some substrate, even though this be nothing more than individual identity or specific nature. Such a common-sense notion of change as involving two elements, the characters that change and the substrate which endures, has been adopted by most philosophers. Marxism, as interpreted by Prezent, appears a second time to proceed by generalizing from a restricted field. It is argued from the fact that sense data relating to material objects are seldom invariant, to the conclusion that all aspects of being are variable. Such procedure would seem to be an illegitimate extrapolation.

The notion that all things contain within themselves opposing elements, antimonies or contradictions, whose interaction leads to change, is one of the most distinctive tenets of dialectical materialism. It is one, as already mentioned, whose Hegelian origin is transparent. The Platonic dialectic is manifested when a first disputant urges one point of view, and is opposed by a second urging the contrary. From their argumentative discourse, a clarification, development and possibly a resolution of the subject of controversy results. Similarly, on the historical scale, Hegel pointed out that one set of ideas tends to call into being an opposing set, and that the two then interact to form a new synthesis. This in its turn forms the thesis for a new antithesis; synthesis occurs again and so the historical dialectic unfolds.

From a historical point of view there is much to be said for the Hegelian method, but the applicability of the dialectic to physics and biology is another matter. For one thing, the notion of antimonies can only be applied metaphorically to inanimate systems, and although it is frequently possible to recognize opposing tendencies in a single object, there is usually no reason to suppose that these opposing tendencies are more fundamental than any other property.

Yet there is a general insistence by Marxists that dialectical science is other than and superior to science based on other philosophies. Bernal[b] states that western science is grounded in seventeenth century philosophy and needs re-interpreting in terms of Marxism. He states that in the Soviet Union, progress has been made in this direction, referring possibly to such work as that under review. Prezent[d] states expressly that Soviet science cannot accept the scientific heritage of western Europe, since the latter is permeated by false philosophical notions. Instead, he states that science must be recast on a basis of dialectical materialism, views echoed also by Fersman.

The difficulty is to discover in what way dialectical materialism can be used profitably in scientific work. If Lysenko's theories are excepted, only very few scientific concepts owe anything at all to dialectical materialism, and up to the present no attempt to recast science on a Marxist basis has gained any general acceptance.

This difficulty seems to have occurred to Levy, who wrote in 1934 that "the so-called laws of the dialectic, couched as they must be in very general terms, must have their principal application in the field of social and economic development. They appear to add little or nothing to the detailed methods of analysis that scientific workers have produced during the past century or so. In a sense, they cannot be expected to add anything to these, for they profess to stand above science. Pure and applied science, technology, and all the social changes that have stimulated and been stimulated by them, must, however, fit into the philosophical outlook of Dialectical Materialism. For science, therefore, it is primarily an interpretative method rather than a method of detailed investigation".

It does seem that any attempt to bring science and dialectics into close relationship results either in a form of scientific theory unacceptable to the majority of scientists, or else makes no difference at all. Lysenko's own attempts to apply the dialectic will be considered later.

The whole method of deriving the Marxian dialectic is open to question. It is surely improper to apply a pattern of historical development to physics and biology without special justification. Moreover, the examples given of dialectical opposites, the wave and corpuscular nature of the electron, the co-existence of catabolism and anabolism and the interdependence of life and death, do not demonstrate that the existence of opposites is fundamental in scientific theory. And, having recognized the existence of dialectical opposites, this does not appear to contribute

much to a further understanding of the nature of the object including them. Opposing tendencies are frequently encountered in science, and can be treated quite easily by normal scientific methods and in the light of realistic or materialistic presuppositions. There is little evidence yet that a deeper understanding of their significance is derived from an application of the philosophy of dialectical materialism.

The last tenet of Marxism to be considered is its emphasis on historical process. This again is a Hegelian trait and shows how much the characteristic tenets of dialectical materialism are derived from historical considerations. It is clear that one of the chief objections by Marxist authors to the vulgar materialism of the French and German schools was its denial of the significance of history and its pessimistic attitude to sociology and human progress. By incorporating the notion that dialectical development or history was an essential attribute of matter, this objection was met even though other difficulties were thereby raised.

Darwin's[a] evolutionary theory, which was published early in the lifetimes of Marx and Engels, was regarded by both as a most important vindication of their theory that matter could only be understood in the light of its history. Yet Engels[a] was critical of several aspects of Darwin's teaching. He writes that "the theory of evolution is, however, still very young, and there is therefore no doubt that further research will modify very significantly the present conceptions, including strictly Darwinian ones, of the course of the evolution of species[1]". According to Komarov, he inclined strongly to the Lamarckian theory that "the need created the organ" when he considered the role of labour in the evolution of man from his simian ancestors, and with regard to Darwin's theory of the struggle for existence, he is quoted as stating:

"I recognize in Darwin's teaching the theory of development, but I only accept this means of proof (struggle for life, natural selection) as the first, temporary, incomplete expression of a recently discovered fact . . .

"The whole of Darwin's teaching on the struggle for existence is simply the transference from the social sphere into the sphere of nature of the teaching of Hobbes on *bellum contra omnes* (the war of all against all) and the bourgeois economic teaching on competition together with the Malthusian theory of population".

Enough has been said to illustrate how the whole philosophical system of dialectical materialism is coloured by its derivation from Hegel's philosophy of history. It is in fact an historicism in the sense of Gilson. It tends to generalize on the nature of being in general from the properties of that part of it which comes under the domain of historical analysis. There is a grave danger in this procedure. The assumption has to be made that historical generalizations, notoriously influenced by subjective bias, are universally applicable to all realms of existence including the domain of biology. This supposition is difficult to substantiate.

The biologist encountering the claim that his data must conform to the philosophy of dialectical materialism as interpreted by Lysenko and Prezent, is not likely to accept this philosophy without considering its validity. He will discover that certain questions, which are for him a matter for experimental investigation, are forejudged and regarded as established *a priori*. In particular, he is asked to accept such conclusions as the non-existence of immaterial being, the eternity of matter, the impossibility of unchanging duration, the existence of fundamental opposites, and the significance of history in biology, all as *a priori*. In fact, all these conclusions are open to controversy, none is self-evident, and some at least are open to experimental investigation. By extrapolating from history to philosophy and deciding philosophically questions which should be approached by experimental methods, Marxists have emptied their philosophy of much of its value. As thus interpreted, dialectical materialism does not deal with the necessary properties of being. Instead it considers a controversial set of properties which may belong to certain aspects of being, but which have never been satisfactorily demonstrated to characterize the whole.

The instances in which Lysenko and Prezent have attempted to argue *a priori* on biological problems, either in setting up their own theories, or in criticizing Mendelian genetics, will be given shortly. It is clear that legitimate generalization from observation is the only valid approach to genetic problems.

The Nutrient Theory

Lysenko's theory of "nutrients" represents his most comprehensive attempt to explain the genetical observations of himself and his followers and to cover genetical phenomena in general.

[1] Die Entwicklungstheorie selbst ist aber noch sehr jung, und es ist daher unzweifelhaft, dass die weitere Forschung die heutigen, auch die streng darwinistischen Vorstellungen von dem Hergang der Artenentwicklung sehr bedeutend modifiziren wird.

The beginnings of the theory were apparent in 1936 but the full formulation did not appear until 1943, when Lysenko[t] published his monograph "On Inheritance and its Variability". Further light on this theory was thrown by Keller's appreciation of it in 1944.

This theory, like most of Lysenko's concepts, may be found in embryo in Darwin's writings. In Chapter II reference was made to Darwin's[b] notion that "excess of food" might well be a potent factor inducing genetic variation, and this idea appears to have been adopted and modified by Lysenko and applied to cover a large number of genetical phenomena.

It is not clear what precise significance Lysenko attaches to the word "nutrient". He appears to use it in a very wide sense, referring to any constituent or aspect of the plant's external environment. Organic structures are regarded as having arisen during the course of biological history by the conversion of individual "elements" of the environment into the living substance of the plant, this process being termed "assimilation"[1].

Great care must be exercised not to confuse Lysenko's terminology with that normally used by plant physiologists. Nutrients for Lysenko are not the simple inorganic substances of the physiologist, although these are included in Lysenko's concept. It has a much wider connotation, and covers environmental factors such as the conditions of existence supposed to determine phasic development, and various general climatic factors such as temperature, rainfall, etc. Similarly, assimilation is not understood to mean the process of absorption and conversion of inorganic substances into the substance of the plant, although this is included. It has a much wider meaning and is understood to refer to the process whereby nutrients of any sort are taken in from the environmemt and converted into the substance of the plant. As Lysenko expresses it, "environmental conditions on being incorporated, assimilated into the living body, become not external but internal conditions, i.e. become part of the living body and for their growth and development come to require those same nutrients and those conditions of environment which once they were themselves".[1] Here it is important to note that Lysenko does not admit the validity of the distinction current in modern genetics between the genotype and phenotype. For Lysenko, the hereditary nature of a plant is equivalent to the plant as a whole. All changes are regarded as affecting the nature of the plant, although it is agreed that the transmission of modifications to the offspring does not always occur. Lysenko insists that the assumption of a stable genetic substrate underlying phenotypic variation is an unproved hypothesis. Because of its comparative immutability, this notion of the genotype is regarded as undialectical.

According to Lysenko,[2] the whole notion of the genotype is an illegitimate abstraction, and he prefers to treat the matter more concretely by equating the nature of a plant with its concrete reality, and not with a hypothetical genetic substrate which interacts with the environment to give the phenotype. "The plant organism, or its individual cells," he remarks, "do not contain any special hereditary substance distinct from the soma of these cells[3]". Lysenko admits the reality of interaction between the environment and the plant, but insists that, by virtue of the interaction, creative development takes place and the nature of the plant continually changes, a concept in accordance with the standpoint of dialectical materialism.

Development for Lysenko[t] is the fundamental starting point. No biological organism is static but continually changing and this continual change is regarded as the expression of the biological dialectic. Lysenko and Prezent insist that development is the outcome of mutual contradiction within the organism, though it is not made clear what exactly constitute the opposing elements. Normal development, which is creative in the sense that new (phenotypic) properties appear, is regarded as arising from the interaction between the plant and its environment, during which the plant assimilates nutrients of like nature to itself. At each stage of development, the combination of the plant with the assimilated nutrients is regarded as giving rise to a compound, different from the preceding stage, and which in its turn combines with fresh nutrients. The vernalization phases of Lysenko are regarded as major steps in this

---

[1] Lysenko's own statement that "hereditary constitution is as it were a concentrate of the environmental conditions assimilated by the plant organisms in a number of preceding generations" has already been noted and compared to the corresponding notion of Burbank.

[2] внешние условия, булучи включены, ассимилированы живым телом, становятся уже не внешними условиями, а внитренними, т.е. они становятся частицами живого тела, и для своего роста и развития уже требуют той пищи, тех условий внешней среды, какими в прошлом они сами были.

[3] в растительном организме, в его отдельных клетках нет никакого отдельного от тела этих клеток специального вещества наследственности.

process. The whole process of dialectical development may be represented schematically as follows:—

Plant stage 1 + appropriate nutrients

↓

Plant stage 2 + appropriate nutrients

↓

Plant stage 3 + appropriate nutrients

↓

Plant stage 4, etc.

Finally, in reproduction, there is a return to the embryonic stage, and the cycle begins over again.

In asexual reproduction, the process is comparatively simple and may be represented as follows:—

Plant stage 1 + appropriate nutrients

.
.
.
.
.
.
.

Plant stage n + appropriate nutrients

Plant stage 1a | Plant stage 1b | Plant stage 1c, etc.

If the environmental conditions are suitable, stage 1a will be similar to stages 1b, 1c, etc., and also to stage 1 of the previous generation. In this case, inheritance is simple in the sense of Timirjazev. Cases in which these stages are different will be encountered, according to Lysenko, when the nutrients are of a certain type.

In sexual reproduction, the scheme is as follows:—

Plant stage 1 + appropriate nutrients | Plant stage 1′ + appropriate nutrients

.
.
.
.
.
.
.

Plant stage n (pollen grain) + Plant stage n' (ovum)

Plant stage 1a | Plant stage 1b | Plant stage 1c, etc.

It will be noted that "in the sexual process where two as it were equivalent cells unite, they assimilate each other mutually[1]" instead of nutrients from the environment, each forming the nutrient of the other. Under appropriate conditions, as in asexual reproduction, stages 1a, 1b and 1c, etc. of the progeny will be similar to each other, and to stages 1 and 1′ of the parents.

It is very important to note that plant development, as understood by Lysenko, is essentially a dynamic cyclical process. He insists that from a materialistic standpoint, nothing remains unchanged; none of the sense perceptible properties of the plant remain constant, even the nuclei undergo continual change, so that it would contradict the essentials of dialectical materialism to introduce into the observed developmental flux some formal immutability, not sense perceptible, such as is represented by the genotype. "The organism", Lysenko remarks[1],

[1] При половом же процессе, когда объединяются две как бы равноправные клетки, обе они обоюдно друг друга ассимилируют.

"beginning with the fertilized egg, as it takes nutriment, is changing, is being transformed and is continually developing new characters, organs, properties and qualities. We say 'new' because in the particular individual a few days before, these characters, organs and properties could not even have existed in any form[1]". The organism for Lysenko does not endure as an immutable entity. It is regarded as a stream of stages, each but a momentary reality and engendering from its assimilative union with the environmental nutrients the following stage. The whole flux of stages is the concrete reality, not a hypothetical static entity, the genotype. Moreover the flux proceeds cyclically, each cycle representing one generation, the similarity between consecutive cycles depending on the assimilation of appropriate nutrients.

This last point is important to notice. The reproduction by a parent or parental pair of offspring like to themselves, i.e. similarity between consecutive developmental cycles, depends according to Lysenko[m], on the plant being provided with suitable nutrients. He states, that living substances have a property which he terms "conservatism", which is their tendency to repeat the developmental cycle of their forbears. This property is believed to be manifested in the power of the plant to select appropriate nutrients for itself, thereby maintaining a form of developmental cycle similar to that of its parents. In Lysenko's[t] own words, "living bodies, according to their nature, select from their environment different conditions, assimilate them, and construct their body according to the dictates of their individual development, i.e. according to their hereditary constitution[2]".

Perhaps no tenet of Lysenko's system has aroused such opposition as his belief in the capacity of plants to select nutrients for themselves, in particular, the selection by ova of appropriate pollen grains, which is but a particular instance of his general theory. It has been said that Lysenko attributes free will to plants and also prescience in anticipating the conditions under which the plant or its offspring will find themselves in the future. Neither of these criticisms is fair, and although Lysenko's metaphorical terminology is very liable to such misconstruction, this difficulty should not prevent every effort being made to appreciate his point of view.

The whole of Lysenko's genetical system is permeated with the Darwinian notion that adaptation to environment is the key to the understanding of all biological variation. Conversely, as Lysenko and Prezent frequently point out, plants, by becoming adapted to certain environmental conditions through natural selection, come, by means of this same process, to have certain biological requirements in respect of the environment. Translating this concept into Lysenko's terminology, plants may be said to need or demand appropriate nutrients, this demand having arisen through natural selection. Extending this concept further, Lysenko states that this demand for certain nutrients may be further sharpened by natural selection, so that, when various nutrients are present, the plant is able to absorb and assimilate those nutrients which are biologically advantageous and to reject the rest.

Such selective absorption and assimilation of nutrients is not regarded as conscious or voluntary, but merely as a biological process that has come about through the operation of natural selection. The second objection that this notion of selection of nutrients implies foreknowledge of what the environmental conditions will be during the ensuing developmental cycle is more serious. But it should be pointed out that, as originally put forward, the theory applied to plants growing in an environment to which they were accustomed. Under such conditions, natural selection might well be conceived as acting, at all stages of the developmental cycle, on the capacity for selecting nutrients. Selection of nutrients at any particular stage might conceivably become modified by natural selection to bring about adaptive changes in other parts of the developmental cycle.

It is much harder to see how this should be possible when the plant is grown in an environment to which it is not adapted. But again, it is not inconceivable that natural selection should so refine the selective power that any departure from the norm in the environment should tend to bring about compensating changes in the selection of nutrients at any part of the life cycle. This point will be mentioned again in the section on selective fertilization.

Lysenko's explanation of the comparative invariability of homozygous lines has now been

---

[1] Организм, начиная с оплодотворенной клетки, питаясь, все время видоизменяется, превращается, развивает все новые и новые признаки, органы, свойства и качества; говорим "новые", потому что у данного же индивидуума еще несколько дней назад этих признаков, органов и свойств ни в какой форме могло и не быть.

[2] Живые тела, соответственно своей природе, избирают из окружающей внешней среды различные условия, ассимилируют их, строят своё тело, согласно закономерностям их индивидуального развития, т.е. согласно их наследственности.

outlined. Attention must now be given to his theory of hybridization, dominance and segregation.

Enough will have been mentioned by now to indicate the emphasis that Lysenko and Prezent place on the environment in developmental and genetical questions. The whole cycle of development is regarded as the outcome of the continual assimilation by the various plant stages of appropriate nutrients, the approximately repetitive pattern of the cycles of homozygous forms being attributed to the property of conservatism. Yet it must be remembered, according to Lysenko and Prezent, that invariability over several generations depends on constant environmental conditions, for as Prezent[b] remarks, "there is no single character of an organism which is autonomously pre-determined and is in its development independent of the conditions of development[1]".

When two plants hybridize, the type of offspring produced—or expressing the matter dialectically, the type of developmental cycle initiated—depends, according to Lysenko[m] and Prezent, on the environment. In the zygote, the potentialities for development of both parents are thought to be present. The actual course of development is regarded as unpredictable until the environmental conditions, or rather the conditions for existence which actually affect development, are specified.

When these conditions are known, it is believed that dominance of any pair of alternative factors can be predicted by the assumption that those characters will be dominant which are best adapted to the environment of the hybrid. Instead of having a single conservative tendency in development, the hybrid is regarded as having a double potentiality. On germination, the hybrid is stated to assimilate those nutrients which cause the cycle of development to proceed in the direction biologically most advantageous. Since there are two possibilities for many characters, there lie open many possibilities for alternative courses of development. At each stage in the developmental cycle where the two possibilities present themselves, it is thought that the plant exercises its selective power and absorbs those nutrients which will shunt the course of development along the most advantageous course.

It will be noticed that Lysenko's theory of dominance is but a corollary of his general theory of development, with the additional assumption that each hybrid is able to develop the characters of either parent, if only placed in conditions where these characters would be biologically advantageous. The evidence supporting this theory has already been noticed. Instances of dominance of characters adapted to the environment of the hybrid have been reported, also cases of F_1 heterogeneity. In neither case is the evidence compelling, and it is clear that Lysenko's theory of dominance is derived more from theoretical considerations than from practical obervation.

One special application of Lysenko's theory of dominance led to debate as early as 1935. This was his conclusion that earliness is always dominant provided that appropriate conditions of existence were applied. His reason for this conclusion, which is in part a corollary of his general theory of dominance, rests on the assumption that earliness is biologically advantageous and is retarded by the presence of limiting factors, which are inoperative when heterozygous. These assumptions have already been discussed. It is hardly necessary to add that Lysenko does not consider his factors as analogous to Mendelian genes, but rather to the limiting factors of plant physiology. If a hybrid is formed from parents having limiting factors in different developmental stages respectively, the assumption is made once again that the F_1 hybrid combines the potentialities of either parent, and since the limiting factors are less biologically advantageous than those regulating unimpeded development, it is assumed that nutrients will be selected so that the course of development is switched in the direction of greater earliness. This process, in which the limiting factors derived from each parent are neutralized by the factors encouraging normal development in the other, is referred to as complementation.[2]

The evidence for the contention that F_1 hybrids cannot be later than the earlier parent has already been given. It depends for its effect on the supposition that all the exceptional cases known are to be explained by assuming that the appropriate biological conditions were wanting. The objections in this appeal to unknown factors have already been treated.

Having considered Lysenko's theory of dominance, it is possible to examine his theory of segregation. As before, Lysenko and Prezent emphasize that the characteristics of the F_2 progeny and the segregation ratios themselves are influenced profoundly by the environment.

---

[1] Нет ни одного признака в организме, который был бы автономистически предопределенным и в своем развитии был бы независимым от условий существования.

[2] взаимопреодоление, literally "mutual conquest".

In his earlier writings, Lysenko regarded F_2 segregation from a hybrid F_1 as inevitable, but later rejected this view as a Mendelian error. According to Lysenko, segregation occurs through differentiation of gametes, the latter caused by local differences in the nutrient supply to the sporogenous tissues. Since it is believed that F_1 hybrids contain the developmental potentialities of each parent, local differences in the nutrient supply to the gametes are regarded as able to differentiate the gametes and thus bring about segregation. The formation by an F_1 hybrid under normal conditions of approximately equal numbers of the various gamete forms is ascribed by Prezent[c] to a mutually-levelling[1] process in which local variation in environmental conditions is supposed to be sufficient to prevent any predominance of any particular gamete type. Lysenko and Prezent support their interpretation of segregation by quoting anomalous cases supposed to depend on environmental factors, also instances of heterozygous F_1 progenies from homozygous parents. It has already been shown that this evidence is not satisfactory. They are also at much pains to mention the numerous examples known of segregation of characters, which do not conform to the simple Mendelian 3 : 1 ratio, also cases of differences in reciprocal crosses. They also refer to Millardetism, the occurrence of non-segregating F_2 hybrid progenies, which has already been considered, and for which the evidence is inadequate to support a new genetical theory. Millardetism is explained by Lysenko as due to the operation of appropriate environmental conditions. It is believed that by the assimilation of appropriate nutrients, all the gametes of F_1 hybrids may be diverted along similar developmental paths, giving rise in consequence to a uniform F_2 generation.

Lysenko's deduction from the theories outlined above that F_2 transgressive segregation for earliness is impossible has been mentioned in Chapter IV. By assuming that earliness is biologically advantageous and necessarily dominant, and by assuming further that segregation is a subtractive process, causing a dispersion of the increased developmental potentialities of the F_1 hybrid, the corollary that F_2 plants cannot be earlier than the F_1 hybrids follows. But since the premises have been shown to lack cogency and the experimental evidence to be conflicting, this rider cannot be accepted without further evidence.

Variation

Up to the present, consideration has been confined to simple cases of inheritance. In these the assumption has been made that the conservatism of the plants concerned has been sufficiently strong to resist the pressure of environmental conditions. Such in Lysenko's opinion, is very rarely the case in nature, where the environment is regarded as continually modifying the genetical constitution of plants, or breaking down their conservatism. The repetition by offspring of the parental developmental cycle is compared by Lysenko[1] to the uncoiling of a spring which has been wound up previously. The sex cells of the parent, according to this analogy, represent the coiled spring and "reflect or accumulate the course of development followed by the previous generations, and especially by the immediate forbears[2]". The development of the sex cells to form the next generation is compared to the uncoiling of the spring and this is regarded as mirroring the winding up process, providing the external conditions are the same. If, however, as is usually the case, the environment changes, "the development of the subsequent generations tends to obliterate the path of the earlier generations, or more exactly, constantly converts it (the path) into a relatively new one[3]".

It has already been mentioned that Lysenko claims that pure lines vary. Moreover, this process is distinguished from mutation, which is regarded as a rare occurrence of little importance. Pure line variation is thought of as a continuous process responding to environmental conditions. Lysenko's adoption of Darwin's notion of continuous variation is very evident and plays an important role in his general theory.

Lysenko[b] admits that pure lines may be morphologically uniform but he asserts that this uniformity is confined to the external morphological features or "shirt[4]" of the plants and does not extend to their physiological characters or "soul[5]". Both Lysenko[h] and Dolgušin[a,c] explain that the progeny of a homozygote becomes differentiated *inter se*, because each plant of the

---

[1] взаимо-нивелелирование.

[2] отражают, аккумулируют пройденный путь развития предыдущих поколений, особенно ближайших предков.

[3] Развитие последующих поколений стирает путь предыдущих поколений, вернее—все время превращает его (этот путь) в относительно новый.

[4] рубашка. [5] душа.

progeny finds itself under slightly different environmental conditions and consequently requires or demands slightly different nutrients in comparison with its fellows. Under altered circumstances plants are held to "assimilate environmental conditions which to some extent do not correspond to their nature, and organisms or individual organs are produced which differ to a greater or lesser degree from the preceding generations[1]". In self-pollinated plants, the family so obtained from a single homozygote will thus come to be physiologically differentiated, even though all the plants have similar shirts. The latter are not regarded as of much adaptive importance.

Besides becoming differentiated, the pure line is believed to deteriorate in respect of its vegetative vigour. Each of its members is thought to become adapted to an extremely narrow range of conditions, outside which it is at a serious disadvantage. Lysenko[l] states that "in each new generation obtained by selfing the range of the adaptive potentialities for development becomes ever narrower[2]". Moreover, union between like gametes is regarded as biologically disadvantageous, since development, which is dialectical in its nature, should spring from the conflict of opposites, which are not provided by the union of similar gametes.

The evidence given by Lysenko for pure line degeneration is not impressive. It gives the impression that his insistence on the fact of deterioration following inbreeding is derived largely from the conclusions of Darwin and Timirjazev, and not from actual experiment. It does not follow from the theory of nutrients without the additional assumption that the assimilation of nutrients and the resultant initiation of cycles of development adapted to new conditions involves at the same time a loss in adaptability to other conditions. It should be noted that segregation of physiologically differentiated representatives of a pure line is regarded as a process of loss in potentialities of development, just as sexual fertilization is regarded as conveying increased potentialities for development. This somewhat arithmetical view of developmental potentiality, which is regarded as doubled by fertilization and reduced by segregation, must be constantly borne in mind.

The theory of rejuvenation is the corollary of the preceding. Lysenko's[b] method of rejuvenating pure lines has already been described. It involves the emasculation of a few plants and their pollination by a pollen mixture derived from other plants of the same variety. It is supposed that, when this is done, the ova of the emasculated plants select those pollen grains, here acting as nutrients, which will give rise to the best-adapted offspring.

It has already been stated that crossing is regarded as beneficial since it doubles the possibilities of development of the hybrid, thereby extending its range of adaptability. By the process of selective fertilization, it is believed that the best combination is effected. Moreover, since the ovum and the grain have different types of soul, although members of the same pure line, they are regarded as vital antimonies, whose interaction can give rise to a vital impulse in accordance with the principles of dialectical materialism. Lysenko[t] remarks that the fertilized ovum contains "all the inherent properties of both parents, and from the contradictions that exist between the two relatively different conjugating cells, there arises, there increases, the internal vital energy, the property of being able to change and be transformed".[3]

The very copious evidence supporting the conclusion that intravarietal crossing gives rise to increased yields has already been reviewed. Though of considerable interest, it reveals more about the genetical heterogeneity of Russian varieties than about rejuvenation in a pure line. The erroneous identification of varieties with pure lines has vitiated much of Lysenko's genetical work, and has obscured many of the controversial issues. It seems very probable that Lysenko's theory of rejuvenation preceded the facts rather than vice versa.

The theory of pure line variability obviously has very important consequences. Since it is assumed that the plant's selective power in choosing its nutrients may result in a deflection of the cycle of development away from the parental type i.e. may overcome the plant's conservatism,

---

[1] ассимилировать условия внешней среды, в той или иной степени не соответствующие их природе, подучаются организмы или отдельные участки тела данного организма, более или менее отличные от предшествующего поколения.

[2] У каждой новой ґенерации, полученной путем самоопыления, все больше и больше суживается круг приспособительных возможностей к развитию.

[3] все породные свойства одной и другой формы. На основе противоречия, получающегося между объединившимися двумя относительно разными половыми клетками, и возникает, усиливается внутренняя жизненная энергия, свойство к видоизменению и превращению.

it follows that by regulation of nutrients, it should be possible to produce new types of developmental cycle at will, or in normal genetic terms, to induce mutation. This of course, is what Lysenko claims, and he goes on to state that one of the most useful results of his theory is the fact that he is able to liquidate the conservatism of plants and to produce thereby new cycles of developments or new varieties. A large body of rather inconclusive evidence in favour of this conclusion has already been reviewed in the section on Induced Variation.

In addition to believing in directional modification of the hereditary constitution of plants, Lysenko[t] also holds that various agencies may unsettle or "shatter[1]" the conservatism of these plants and render them more sensitive to factors inducing variation. This notion is found also in Mičurin's writings. The breaking down of the conservatism of plants is envisaged in terms of the nutrient theory. It has already been noted that plants are regarded as reproducing unchanged—or rather the cycles of development are regarded as approximately reproducing themselves—whenever the nutrients are suitable. But if the nutrients are quite unsuitable, the conservatism of the plant may be overcome and a modified cycle of development ensue. This would occur through the assimilation of new nutrients and the establishment of fresh compounds between the plant and its nutrients. Or it may be that the conservatism of the plant is altogether broken down or shattered. The plant then loses for the time being its capacity of recapitulating the developmental cycle of its forbears. In this state, the plant is regarded as extremely susceptible to modifying factors, and so extremely variable. Conservatism is regarded as regainable if the affected plant is grown for several generations under constant environmental conditions.

Lysenko[t] enumerates three agencies able to effect shattering: adverse environmental conditions, hybridization and grafting. Both he and Dolgusin[c] refer to many of the instances in which the environment is believed to have brought about modification in the varietal type as instances of shattering. As examples Lysenko mentions the conversion of Kooperatorka, Novokrymka 0204 and Stepnjačka winter wheats into spring forms by spring sowing. He remarks also that spring wheats derived in this way tend to revert to winter forms. This he attributes to the fact that conservatism is not immediately re-established. Work on grafting only came into prominence in Lysenko's programme of research in 1940, but has since been used as the most telling of his evidences. Lysenko appears to interpret his results in two ways, either regarding the interaction between stock and scion as an instance of heritable modification, or describing it as vegetative hybridization. Whether these two methods of explanation are regarded as self-exclusive is not clear, but it seems likely that they are not. Lysenko defines a hybrid as an individual having the properties of two breeds, and expressly includes vegetative hybrids.

The interpretation of the claimed interaction between stock and scion is as follows. Lysenko[t] emphasizes firstly that in graft hybridization, from which category he excludes chimaeras such as *Cytisus Adami* Poit. and *Craetegomespilus* sp., the only material connexion between the fruits of the scion and the stock is the xylem sap. In this fluid, which rises through the stock to the scion, he states that substances possessing heritable properties are dissolved, for "any small live part or even drop of the body (if the latter is a fluid) possesses the property of inheritance".[2] When these dissolved substances reach the scion, they are believed to act as nutrients, and to be assimilated to form a new compound, from which a fresh developmental course proceeds. This concept has a very close affinity with Burbank's notion of sap hybridization from whom Lysenko may have derived this explanation.

This assimilation clearly involves a weakening of the conservatism of the scion. It is possible that in other cases, hereditary modification may be caused by the stock without assimilation of nutrients. These latter cases could be regarded as instances of induced variation. Since the term nutrient has such a very wide connotation in Lysenko's terminology, it would be difficult to distinguish exactly vegetative hybridization from induced mutation.

The evidence for interaction between stock and scion has already been considered, and although much of it is highly unsatisfactory, it is sufficiently suggestive to require further investigation. Only in this way can a conclusion be drawn as to whether heritable changes are caused in this way. Lysenko's interpretation of the facts is but another corollary of his general theory of nutrients, assuming in addition that nutrient substances derived from the stock are

[1] расшатывать.

[2] Любая живая частичка или даже капелька тела (если последнее жидкое) обладает свойством наследственности.

present in the xylem sap. It is not clear what explanation Lysenko intends for the reciprocal effect of scion on stock, though conceivably this might be effected via the phloem.

A final consequence of Lysenko's theory of nutrients is the conclusion that a single plant may be genetically heterogeneous—or in dialectical terminology, may pursue different developmental tracks in different parts. According to Lysenko, it is to be expected that a plant should develop uniformly throughout its extent only if the nutrient supply is uniform, if the selective powers of all parts of the plant are similar, and if the conservatism of the plant is sufficiently strong to resist internal differentiation.

It has already been shown that Lysenko explains segregation in the gametes on the assumption that local differences in nutrient supply occur in the sporogenous tissue. Such local differences are not supposed to be confined to the reproductive organs, but to occur anywhere throughout the plant wherever different nutrients have been selected by the various parts. The capacity of these different parts for selecting nutrients in accordance with local requirements is regarded, as before, as having arisen through natural selection.

This belief in the possibility of internal variation is a simple corollary of the theory of nutrients and involves no additional assumptions. The evidence for it has been reviewed in the sections on Internal Genetic Variation and Mixed Inheritance. The data need further confirmation before acceptance.

Selective Fertilization

Owing to the large place that the theory of selective fertilization has played in the Russian genetical controversies, it must be treated in some detail. It has been already pointed out that selective fertilization is only a special instance of the general property of selective assimilation of nutrients, which Lysenko[t] attributes to all living matter. Its origin is attributed to natural selection as in the case of selective assimilation in general. The term selective fertilization for the process of selection by the ovum of biologically advantageous pollen grains is that used with a different significance by the American botanist Jones. At first, Lysenko and Prezent referred to this process as "marriage for love[1]", a term they found expedient to drop in their later publications. The derivation of the theory seems to go back to Darwin's[c] notion of pollen prepotency, and has points in common with some of Mičurin's ideas on the horticultural value of pollen mixtures. But Prezent insists that selective fertilization is not the result of pollen tube competition but an actual choice by the ovum.

Much of the evidence for the theory has already been reviewed in the section on Rejuvenation, a process which Lysenko explains as due to selective fertilization. In view, however, of the probability that the increased yields obtained by intravarietal crossing are comparable to those obtained in wider crosses, attention will be turned to the other evidence put forward to support the theory.

In 1939, Brežnev[b] published the results of his experiments on tomatoes. He emasculated the variety Stoffert and pollinated it with a mixture of self pollen and pollen from another variety. All the offspring were hybrids. He next tried the effect of self-pollinating Stoffert and adding foreign pollen twenty minutes later. Again, all the offspring were hybrid. Finally, he pollinated Stoffert with a mixture of pollen from other plants of the same variety and foreign pollen, and this time obtained 22% of the F_1 generation of the maternal type. These experiments were interpreted as evidencing selective preference by the Stoffert ova against their own pollen. Later experiments by Breženev[c] with the tomato variety Best of All showed that the yield obtained after intravarietal crossing was positively correlated with the number of plants providing the pollen mixture. This result again was ascribed to the operation of selective fertilization.

A whole series of experiments on selective fertilization appeared in 1940. Jablokov showed that open-pollination of species of *Juglans* L., *Populus* L. and *Corylus* L. gave progeny more vigorous than those obtained from selfing. Conservation of the maternal morphological type was claimed. Lesik investigated maize, intermixing yellow and white grained varieties. The latter gave rise to progenies with less than 10% and sometimes less than 1% of yellow grains. Other experiments with maize were made by Kravčenko. He intermixed twelve varieties, and observed a certain number of cases in which varietal purity was maintained. Novikov examined the behaviour of interspecific crosses of *Vicia Faba* L. and other *Vicia* spp., both in simple crosses and with pollen mixtures. He found that the maternal plants were fertilized with pollen from plants similar to themselves morphologically. Like the other authors quoted, he felt compelled to ascribe his results to the effect of selective fertilization.

---

[1] брак по любви.

Lysenko[t] has drawn special attention to the results obtained by Dolgušin, who sowed twenty winter wheats at Odessa, emasculated them and allowed them to interpollinate. He claimed that by so doing, the F_1 and F_2 generations obtained were hardier than the controls although the maternal morphology (shirt) remained the same. Plants were reported hardier than Lutescens 0329, the hardiest variety used in the experiments. Lysenko[q] also mentioned a case in which *Prunus Besseyi* Bailey was pollinated for five generations with peach pollen, yet remained true to the maternal type. Later, in 1943, Lysenko[t] alluded to experiments made by Avakjan, who intermixed spring and winter rye varieties and obtained thereby three generations of almost exclusively spring forms.

Hačaturov[a] believed that the capacity for selective fertilization was developed to varying extents in different plants. He inter-planted green and yellow grained rye varieties and found that the proportion of yellow grains occurring in the green-grained varieties tended to remain constant over a period of two years.

Similar results were reported by other authors in 1941. Drobinskiĭ crossed grey and yellow seeded forms of *Brassica juncea* Coss. var. *sareptum*, and since many of the F_2 progenies failed to conform to a 3 : 1 Mendelian ratio, attributed the discrepancy to selective fertilization. Gluščenko[b] performed experiments on intervarietal crossing in rye, which he claimed produced offspring that survived the winter better and had larger ears and heavier grain than the controls. Only in a few cases were the shirts affected. Both Gluščenko[c] and Kobeljkov worked with buckwheat and found that open pollination in varietal mixtures resulted in offspring that tended to be earlier, taller, more vigorous and with a higher seed productivity than the controls. The shirts remained unaffected.

Jakovuk[b] reported an extensive series of experiments with tobacco. The variety Tyk-Kulak 92 was pollinated with a mixture of self pollen and pollen from the varieties Dubec, Trebizond 1269 and Samsun 5. Very few offspring of the maternal type were produced. Next, Samsun 57 was pollinated with a mixture in various proportions of self pollen and pollen from American 572, Trebizond 1269 and Tisskiĭ. In this case, most of the fertilizations were effected by pollen from American 572, even when the other pollen was up to five times as abundant as the latter. Lastly, American 573 was pollinated with a mixture in various proportions of self pollen and pollen from Samsun 57, Trebizond 1269 and Tisskiĭ, and in this case, the majority of fertilizations were effected by Tisskiĭ. Jakovuk concluded that Samsun 57 tends to select American 573 although the reverse combination is incompatible. It was reported also that Trebizond tends to select Samsun 57, this combination also being incompatible vice versa. Trebizond selects its own pollen when mixed with Maryland. No correlation was found by Jakovuk between these results and the germination behaviour of the various pollen types on artificial media.

Compared with Jakovuk's detailed researches, the other reports on selective fertilization in 1941 are less important. Musiĭko tried the effect of supplementary pollination in a series of crop plants and reported yield increase in centners per hectare of the following magnitudes: maize, 5·21; sunflower, 2·34; hemp, 1·2; lucerne, 0·64; rye 2·0; buckwheat, 2·3; and millet, 3·0. It was believed that supplementary pollination increased the chance of selective fertilization. Potašnikova[a] grew the wheat Starorusskaja among 22 less hardy wheats. After emasculating it and allowing open-pollination to occur, she reported that the offspring were hardier than the controls. Experiments with rye were made by Rudnickiĭ and Gluhih, who found that the varieties P-13 and Vjatka produced offspring with increased tillering capacity, higher grain weight and larger grain yields after open-pollination. The principal morphological characters of the maternal parents were retained. Both Ponomarenko and Trofimec examined the effect of open pollination on crop yields. The former claimed that the winter survival of Skala wheat was increased thereby, and the latter reported yield increases up to 800% in the leaf and seed production of *Allium fistulosum* L.

Later work on selective fertilization seems to have been more sporadic. Kostjučenko made experiments with rye mixtures and sowed the yellow-grained variety Bezenčuk 1 close to a local winter rye. The offspring were found to have large green grains, to yield more than the parent and to be more winter hardy. These results are ascribed to selective fertilization. Puhaljskiĭ and Krotov[b] have studied intervarietal open pollination in rye and buckwheat. In both cases, yield increases were reported which are regarded as consequent on selective fertilization. Finally, reference should be made to an anonymous[d] report in 1943 dealing with wheat and rye. Supplementary artificial pollination of varieties was found to give rise to progenies that were more winter hardy than the offspring even though the supplementary pollen was derived from less hardy varieties. Conservation of the shirts was reported.

Enough has now been given of the evidence relating to selective fertilization to analyse its cogency. Various general remarks on the subject by Dolgušin,[c] Kolj and Lysenko[n,o] do not add materially to the picture already presented and need not be considered in full.

Firstly, the initial objection, which has already been mentioned, namely, that ova are incapable of conscious choice, must be referred to again. Lysenko's[b] terminology is certainly highly metaphorical. Each pollen grain in a pollen mixture is referred to as a "swain[1]", and after pollination has been effected Lysenko instructs his followers to "let the stigma take whatsoever gamete it wishes[2]". Literally, this is gross anthropomorphism as Žebrak has insisted. But it has already been shown that, in spite of his figurative vocabulary, Lysenko probably does not mean to attribute conscious volition to ova but merely the power of selecting suitable nutrients. The objections levelled by Kozlov and Matveev, Konstantinov *et al.*, Lazarev and Rjabov against the notion of marriage for love seem in part to arise from a too literal interpretation of Lysenko's terminology.

Far more to the point is the objection that selective fertilization attributes the plant with foreknowledge of the environmental conditions under which its offspring will grow. It cannot be denied that Lysenko's[o] statement that "through selective fertilization the progenies are as it were predestined to become adapted to their future conditions[3]" has a curious ring. If the assumption is made that selective fertilization is biologically advantageous, whatever the environmental conditions under which the offspring are grown, the theory could be rejected outright as absurd. That Lysenko tends towards such a position is undeniable. But on the other hand, he frequently describes his theory in such a form that a more reasonable interpretation can be put upon it. This is when the parent and offspring are understood to be growing under similar environments. The theory so limited is much more plausible. There is no *a priori* objection to be levelled against it on grounds of implied foreknowledge in the parent, since it is not inconceivable that such a limited selective power could come into being through natural selection.

Having thus dealt with *a priori* objections to Lysenko's theory, it is possible to inquire whether in fact the data adduced in support of the theory are compelling. To this question, it is difficult to come to any answer other than a decided negative. Excepting Jakovuk's[b] experiments, most of the evidence is insubstantial and can be interpreted quite readily without recourse to the theory of selective fertilization. In order to demonstrate the theory at all satisfactorily, it would be necessary to make cytological and physiological studies of the behaviour of pollen tubes in the ovary and to show that the ovum plays an active role in determining which of several different kinds of pollen grain should fertilize it. Secondly, it would have to be established that the vigour of the progeny obtained from mixed pollination was superior to that derived from simple pollinations of the same parent by any of the pollen types not utilized in the mixed pollination.

No attempt seems to have been made to satisfy the first requirement. The nearest approach to it was made in Jakovuk's experiments, where the behaviour of germinating pollen tubes was studied, not in the ovary of the maternal parent, but on artificial media. Some approach has been made towards satisfying the second requirement, but in most cases the progenies have been compared, not with all the other possible combinations, but only with the original parental type. Moreover, it is not clear what degree of significance is to be attached to the results in the absence of statistical treatment of the data.

Experiments that have been made to examine the theory critically have not confirmed it. H džinov investigated the progenies obtained from mixed pollinations of maize, but found no coarelation between the type of pollen grain effecting fertilization and the biological adaptability of the progenies. Similar results were obtained by Harečko-Savickaja for beet, where again it was found that the pollen grain in a mixture that actually fertilized the ovum was not necessarily that one which would have given rise to the most vigorous progeny.

In view of these findings it is impossible to regard the theory of selective fertilization as anything more than a rash conjecture. In its restricted form the theory cannot be refuted *a priori*, but there is no evidence to suggest that it is true.

---

[1] парень.

[2] А дальше рылце пусть берет какую хочет гамету.

[3] вследствие избирательности оплодотворения как бы "предопределяется" приспособляемость потомства к будущим условиям.

Creative Selection

A minor theme that runs through Lysenko's genetical system is the insistence that natural selection is not only eliminative but also creative. The point of view is also advocated by several Russian geneticists not of Lysenko's school.

Darwin[a] himself stated that natural selection was an eliminative force. But since he believed that the type of variation in a plant was conditioned by the environment, there is a sense in which he could be said to regard natural selection as creative, or at least formative.

Poljanskii has developed this point of view in his criticism of Punnett's evolutionary theory, expressing himself as follows:—

"The conception that selection, by the summation of differences, is by this very act a factor historically creating organisms in the form that we see them in, with all their peculiarities, is quite foreign to Punnett. But if genotypes constitute the product of historical development in the process of prolonged selection, then it is clear that also the variation itself, which is different in different organisms, is conditioned by their whole previous history, i.e. by the process of natural selection. In this sense it is undoubtedly the creative basis of evolution. However, Punnett, like other anti-Darwinians, obstinately refuses to understand this truth[1]".

When taken in this sense, the notion of natural selection as a creative force is but a corollary of the theory that mutation and natural variation arise adaptively in response to environmental conditions, or in Lysenko's terminology, through the assimilation of fresh nutrients.

Insistence on the creative role of selection is apparent in the writings of Lysenko, Prezent[f], Kamšilov, Popov, Sinskaja and Iljinskii. In part this insistence seems to be no more than attachment to a verbal formula. For the rest, it concerns the theory of adaptive mutation which has already been considered. With all these authors, there is an evident distaste for evolutionary theories in which natural selection is assigned to a minor role in evolution. The views of authors such as Willis, Heribert-Nilsson and Turesson are strongly criticized.

Analysis

In the preceding sections of this chapter, an attempt has been made to present the theoretical construction of Lysenko's genetical system in its entirety, without troubling to consider its validity. It is only to be expected that it will appear strange to those unfamiliar with Russian genetical literature. The divergence between Lysenko's theory and present-day Mendelian genetics is very deep, and makes an examination of Lysenko's basic concepts most necessary.

Many of the facts and generalizations which Lysenko has used to substantiate his theories have already been shown as lacking in cogency. But in examining scientific hypotheses, logical cogency is not the only criterion. There are in fact three types of scientific hypotheses in vogue today. Firstly, there are verifiable hypotheses, predictions for instance which can be verified by experiment. Secondly, there are hypotheses unverifiable owing to the impossibility of having made sufficient observations to gurantee their authenticity, as for instance most phylogenetic speculations. And thirdly, there are essentially unverifiable hypotheses, which invoke causes beyond human perception, which may indeed enable an explanation to be given of scientific observations and enable predictions to be made, but which, owing to the inaccessibility of the causes invoked, are undemonstrable.

Essentially unverifiable hypotheses have played a very important role in modern physics, but in biology their scope has hitherto been more limited. There is moreover grave doubt as to the extent and circumstances under which such hypotheses should be used. Pragmatically they have proved most valuable, but this does not justify their being erected into quasi-certainties in a scientific system.

The root difficulty with regard to essentially unverifiable hypotheses is that, when two alternative hypotheses are brought forward, both of which accord with the facts, there is no way of deciding between them, except by performing further experiments in the hope of obtaining facts

---

[1] Представление о том, что отбор, суммируя изменения, тем самым является фактором, исторически созидающим организмы такими, какими мы их видим, со всеми их особенностями,—абсолютно чуждо Пённетту. Но если генотипы представляют сбвой продукт исторического развития в процессе длительного отбора, то ясно, что и характер самой изменчивости, различный у разных организмов, обусловлен всей прошлой их историей, т.е. процессом естественного отбора. В этом смысле он представляет собой, несомненно, творческое начало эволюции. Однако Пённетт, как и другие антидарвинисты, упорно не понимает этой истинны.

which disprove one or possibly both alternatives. If there is no discrepancy between the facts and either hypothesis, there is no way of deciding between them at all.

Usually when this happens, recourse is had to Occam's Razor under the form of the Simplicity Postulate, enunciated by Jeans as "of two alternatives, the simpler is likely to be nearer the truth". But there is no absolute criterion of simplicity, and what may appear simple at one time may be shown to be highly complex later. The application of the Simplicity Postulate is in fact only an exercise in private judgment.

The logical hiatus between fact and generalization on the one hand and the essentially unverifiable hypothesis on the other, makes this undesirable situation inevitable. Now it will be seen that some at least of the Russian genetical controversies have hinged about essentially unverifiable hypotheses and are consequently insoluble.

Fortunately, although essentially unverifiable hypotheses cannot be proved, they can be disproved by the discovery of a single discordance between their implications and the observed facts. Thus, although a positive progress in the construction of hypotheses is impossible, a form of negative progress by the elimination of false hypotheses is possible.

Before considering Lysenko's theories in detail, it is necessary to refer once again to what have been called in this bulletin elastic hypotheses. These are hypotheses which, in virtue of their form, may be extended to cover all facts either known or yet to be discovered, irrespective of the truth value of the hypothesis. The concordance in this case between fact and hypothesis arises, not from a real correspondence, but because the theory is constructed in such a form that it can always be extended to fit any concrete situation. In most cases, this possibility arises because the theory refers to unknown factors which can always be invoked to cover a discrepancy. Such theories being without a basis for verification are of very little use and their truth value is unascertainable.

Lysenko's theory of plant development, which is one of the principal bases of his genetical theory, is claimed to be grounded in the philosophy of dialectical materialism. Prezent's interpretation of this philosophy has already been considered in reference to biological problems, and has been criticized as stating *a priori*, conclusions which, if valid, can only be reached by observation. It is now possible to explain this objection more precisely. Dialectical materialism, as interpreted by Lysenko and Prezent, contains a number of essentially unverifiable hypotheses, in particular the denial of immaterial existence, the denial of the possibility of unchanging duration, the supposition that matter inevitably enshrines fundamental opposites, and the supposition that change is fundamentally historical. None of these postulates can be proved, while all are only doubtfully applicable to biological systems.

Lysenko's theory of development as a cycle of stages is dialectical in the sense that no enduring substrate is admitted. It has already been mentioned that there is considerable difficulty in discovering what exactly is meant by the Marxian concept of change. In the absence of an enduring substrate, there would appear to be no change in the strict sense, only a succession of disjunct states. The recognition of enduring objects such as individual organisms would be denied from this point of view, although Lysenko does not appear to go thus far. Yet by admitting the existence of the organism, something only grasped by the intellect and certainly not a primary sense datum, Lysenko appears to be deserting his interpretation of materialism for realism. If he can concede the existence of enduring organisms, he is not consistent in denying other enduring forms[1] such as the genotype. Lysenko is in fact wavering between materialism and realism, denying the duration of some forms and admitting the existence of others, without realizing his inconsistency.

It may be asserted, of course, that the duration of forms is recognized by many exponents of dialectical materialism. In this case, the criteria by which some forms are admitted while others such as the genotype are denied, would be required. Neither the organism nor the genotype are material *per se*, but are only forms found in material things, and if such forms are accommodated by dialectical materialism, it would appear that it is concurring on this point with the commonsense realism of the Platonic tradition. Lysenko's inconsistency with regard to forms and scientific concepts only reflects what appears to be a similar inconsistency in his philosophy, an unfortunate defect which necessarily injures his whole conceptual framework.

By denying the possibility of enduring genotypes, Lysenko is landing himself in absurdity.

[1] "Form" as used here denotes a quiddity existent in material things. Practically all biological terms such as organism, genotype, root, shoot, leaf, tissue, xylem, parenchyma, cell, chromosome, polyploid, inversion, gene, etc., are forms. They are not sense data but are only cognizable by the intellect interpreting sense data. The manner in which these forms are constructed is one of the most recalcitrant problems of philosophy and psychology.

The phenotype or appearance of an organism is not specified solely by the environment, but by the interplay of environment and the intrinsic nature of the plant, i.e. the genotype. The existence of many different genotypes is demonstrated by the fact that quite different organisms may be found under similar environmental conditions.

Lysenko's concept of life as a flux proceeding in developmental cycles (generations) through assimilation of nutrients is true as far as it goes. But his whole notion of nutrients is diffuse and unsatisfactory. Nutrients, for him, may be either inorganic substances, environmental conditions, whole organisms such as gametes, or organic substances such as are supposed to pass from stock to scion in grafting. The term assimilation, which is used in connexion with all four of these types of nutrient is similarly vague. It may mean assimilation in the physiological sense, i.e. the absorption of chemical substances and their conversion into the living substance of the plant. It may mean an adaptive reaction to environmental conditions, so that, by growing a plant under cold conditions for instance, it becomes more winter hardy. This would be termed assimilation of environmental conditions, the low temperature being regarded as in some sense passing over into the plant and becoming part of it. It may mean the absorption of chemical substances and an accompanying adaptive change in the genetic nature of the plant. It may mean the fusion of gametes in sexual fertilization. And it may mean the introduction into the plant of cytoplasmic elements (including virus particles) with an accompanying change in the genetic nature of the plant. The attempt to treat all these processes under a single heading has led to speculations bordering on the fantastic. Lysenko's notion of nutrients is a confused one, and should have been carefully resolved into its constituent elements before being used in genetical theory.

Lysenko further assumes that nutrients exert a profound effect on the genetic nature of plants. He regards each stage in development as a compound between the preceding stage and the nutrients it had absorbed. This conception again reveals confused thinking. The notion of addition is meaningless unless the terms added and the sum total are of similar natures. In most cases, as Lysenko uses the term, assimilation is between entities different in kind, so that the whole notion of a compound between them is ridiculous.

Going on to Lysenko's next fundamental notion, that of selective capacity, the situation is no more satisfactory. Lysenko asserts that all living substances have the capacity for selecting those nutrients which are of biological advantage to itself and to its offspring, this power having arisen through natural selection. This theory appears false and contrary to the facts. In certain cases it may be that a tissue can absorb nutrients selectively, but as a generalization, especially as applied to pollination in the theory of selective fertilization, it is hardly defensible. Moreover, the whole notion of biological advantage, which Prezent carefully distinguishes from agronomic advantage, lacks precision. M. M. Zavodovskiĭ has pointed out how the concept of biological requirement is liable to be no more than a subjective interpretation on the part of the experimenter. It is quite possible that those factors conducive to vigorous growth in one part of a plant may fail to benefit the plant as a whole. Parents may compete with their offspring and the latter with each other, and the species as a whole may compete with other species, and is itself subject to the operation of unfavourable environmental factors. The biological well-being of the part, individual, and species are frequently served by different factors, and are often in various degrees mutually incompatible. Lysenko's theory of selective capacity should have stated more precisely what is meant by biological advantage.

An illegitimate return to the notion of the genotype is encountered in Lysenko's theory of conservatism. Having rejected the notion of enduring genetic constitution, Lysenko was left without any explanation of the resemblance between parents and offspring, and was forced therefore to devise a substitute. He suggested that living matter was conservative and tended to develop along similar lines to its parents, unless this conservatism were modified or shattered by external agencies. No explanation is given of the way in which the conservatism operates, and it is difficult to avoid the conclusion that the idea was introduced solely to gloss over the deficiency of the theory of nutrients. Conservatism is hardly a property that admits of experimental investigation. It is an essentially unverifiable hypothesis devised to explain the likeness of parents and offspring, and is a metaphysical notion in the Marxian sense. As such it will not be considered further.

Several of Lysenko's practical deductions from his theory were made on the assumption that, in hybridization, the developmental capacities of both parents are present in the zygote. Conversely, in segregation, it is supposed that these capacities become dispersed, F_1 hybrids thus being more adaptable than their parents or the F_2 and following generations. This assumption is untrue in many and perhaps most cases. Many of the properties of a parent may not be

transmitted at all to the offspring, especially if they had depended on a particular gene complex. Furthermore, in segregation, parental properties not potentially present in the F_1, may reappear. As stated above, this theory of additive hybridization and subtractive segregation can be rejected straight away as inconsistent with the facts. Nevertheless, Lysenko and Prezent have attempted to support it by means of the elastic hypothesis that the theory is true if only the plants are placed under appropriate, but usually unknown, environmental conditions. Such elastic hypotheses are valueless.

The theory that individual plants differ genetically throughout their extent, owing to local differences in nutrients, is another deduction from the general nutrient theory that is of dubious validity. In few cases has any internal variation been demonstrated, still less correlated with local differences in the nutrient supply. Lysenko's theory of segregation based on presumed differences in the nutrient supply to the sporogenous tissue is similarly another deduction from this general theory and quite unsupported by evidence.

The notion of pure line variability is fundamental to Lysenko's genetical system. In one sense, Lysenko's contention has much to commend it. The pure line principle has probably been over-emphasized in the past by plant breeders. Much useless effort has been expended in trying to achieve a standard of genetical purity which many breeders today regard as unattainable. The desirability of such a degree of purity, even were it attainable, is also being questioned by many modern breeders, and a certain amount of genetical variability is coming to be regarded as an advantage rather than a defect, always provided that it is accompanied by the maximum of purity as regards important economic features such as quality, disease resistance and time of maturity. Lysenko's attack on the pure line theory is not made from this angle, however, but is intimately bound up with his notion of development as a process arising from the assimilation by the plant of nutrients to give new plant stages. The agreement between this theory and the facts is poor, especially as Lysenko specifically excludes mutation from the mode of variation that he postulates. Much of Lysenko's argument is, moreover, vitiated by his unfortunate confusion between pure lines and existing agricultural varieties. There is no satisfactory evidence that pure lines vary in the way suggested by Lysenko, nor that they necessarily degenerate on selfing.

Lysenko has attributed the vigour often found in hybrids to the interaction of dialectical opposites, and conversely has explained inbreeding depression as arising from the lack of mutual contradiction. This is a clear case of endeavouring to import *a priori* notions from his interpretation of dialectical materialism into genetical theory. It has already been shown that Lysenko's concept of dialectical antimonies is an essentially unverified hypothesis inappropriate either to philosophy or science. This theory therefore need not be considered further. It may be maintained, of course, that the notion of dialectical opposites is valid, but that Lysenko has misapplied it. The consideration of this possibility, however, lies beyond the scope of this bulletin.

A serious general criticism which applies to all aspects of the nutrient theory is that it has not been investigated physiologically, in spite of Lysenko's constantly reiterating Timirjazev's dictum that genetics is but a branch of physiology. Nutrients are invoked to explain every kind of genetical phenomenon, but very seldom is any attempt made to specify what the relevant nutrients are, or what is the mode of their action. In most cases, they are purely hypothetical entities for whose existence there is no real evidence. It must also be pointed out once again that the evidence for most of Lysenko's theories is extremely unsatisfactory. Hardly a single controversial tenet of his genetical theory is supported by facts beyond question, and this alone raises grave doubts as to the validity of the whole system.

Lastly, it will have been observed that throughout the exposition of Lysenko's theory given above, practically no reference is made to any of the genetical work done by Mendelian geneticists in the past forty years. The chromosome theory of genetics and the numerous experiments on cyto-genetical correlations have been almost completely ignored by Lysenko's school. It is true that Kostrjukova and Černojarov, and Kružilin discuss the effect of colchicine on chromosomes, but they conclude that the distinctive characteristics of polyploids are not caused by the duplicated chromosome complement. In other cases, chromosomal observations are held to be secondary effects not determining morphological differences. Sipkov asserts that chromosome number is a variable property and markedly influenced by the environment. Lysenko has concentrated on a small number of dubious experiments and erected upon these his own genetical theory, making no attempt to cover the enormous body of well-established data collected by Mendelian geneticists the world over. This scientific obscurantism is one of the strangest characteristics of Lysenko's system and cannot but help to generate distrust of a theory so circumscribed.

Enough has been said already to make it clear that Lysenko has derived his system, not by generalization from genetical data, and the elaboration of an explanatory hypothesis to cover his generalizations, but by the elaboration of an *a priori* theory derived principally from literal interpretations of the writings of Darwin, Timirjazev, Mičurin and Burbank, these synthesized as far as possible with the philosophy of dialectical materialism. The importance attached by Lysenko and Prezent to alogical forms of scientific discourse has already been discussed in Chapter III. From a scientific point of view, the conspicuous alogical elements of Lysenko's system can be completely ignored. The remaining logical elements have been analysed in the last two chapters, with the conclusion that they are lacking in cogency.

VI. ANTI-MENDELISM

Perhaps more than half the energies of Lysenko and his school have been expended, not in elaborating their own system, but in attempting to overthrow modern Mendelian genetics. So characteristic is this latter objective that no survey of Lysenko's theories would be complete without a consideration of his arguments against genetics as ordinarily understood. The methods of attack employed are various, and include an even greater proportion of alogical considerations than their constructive theories. All the alogical forms of discourse noted in Chapter III are used repeatedly and will be noted briefly in the following sections. The logical objections will be considered afterwards.

Authorities

Mendelism has been frequently attacked for its real or supposed conflict with the recognized authorities, viz. dialectical materialism, Darwin, Timijazev, Mičurin, Burbank and Lysenko.

The opposition between Mendelism and the philosophy of dialectical materialism has been one of the most frequent objections raised by Lysenko[m,t] and Prezent[e], and is one that must be carefully considered. These authors, together with Jakovlev[a], insist that the relatively stable forms, such as the genotype, postulated by Mendelian genetics are undialectical, since according to the philosophy of dialectical materialism, everything is in a continual state of flux. Lysenko[b,m,n] argues similarly against the pure line concept of Johanssen. Lysenko and Prezent[e] state that the conclusion of geneticists that segregation is independent of environmental conditions is also undialectical, since it denies the universal interaction of all material things and postulates an identity in behaviour contrary to the tenets of dialectical materialism.

Žebrak[b,d] has attempted to defend genetics from these charges though not entirely successfully. He has stated that the fact of gene mutation proves the mutability of the genotype and removes the ground of Lysenko's criticism. Lysenko[j] points out, however, that mutation is a comparatively rare event and would only introduce a number of sporadic changes in genetical constitution instead of the continuous variability which he regards as alone consistent with dialectical materialism.

Mangelsdorf in America, commenting on the charge that genetics is anti-Marxian, finds no reason to suppose that it is. Muller similarly finds no inconsistency between dialectical materialism and Mendelism. There are, of course, several different interpretations of dialectical materialism, that favoured by Lysenko and Prezent being quite clearly inconsistent as a whole with Mendelian genetics. For scientists, however, no unfounded or speculative philosophical views can be allowed to intrude into scientific problems, so that the whole controversy as to whether or not genetics is congruous with one or other of the various interpretations of dialectical materialism is a matter of indifference to them.

A like indifference will be felt in face of the objection raised by Lysenko,[j] Prezent,[b,e] Meĭster[b] and Maštaler, that genetics is anti-Darwinian. There are no grounds for supposing Darwin to have been infallible on any topic, and any inconsistency between any of his theories and those current in genetical science today is a matter of little consequence. Lysenko[j] and Prezent[f] are fond of representing Mendelism as the clerical reaction to Darwinism, a pleasing fancy which should entertain many modern geneticists. It seems unnecessary to quote further examples of this attitude, or to consider the claim of Kostrjukova and Černojarov that modern Mendelian theory is inconsistent with Mičurin's theory of the interaction between the plant and its environment.

Heresies

Mendelism has been accused by Lysenko and his followers of involving one or other of the heresies recognized by himself, viz. metaphysics, capitalism, fascism or abiologism.

The charge that genetics is metaphysical or formal is merely the converse of the accusation that it fails to square with dialectical materialism. A long series of articles by such writers as Lysenko,[b] Prezent,[b,e] Kolj, Meĭster,[b] Šlykov, Maštaler, Poljanskiĭ and Keller have reiterated this charge, the ground being that geneticists, by illegitimate abstraction, have postulated the existence of relatively stable enduring forms such as genes and the genotype. These are regarded as undialectical.

Engels[a] is frequently quoted in this connexion, especially those passages in *Anti-Dühring* in which he endeavours to prove that life is impossible without the conflict of dialectical opposites. He stated that "every organic being is at every moment both the same and not the same; at every moment it utilizes matter brought in from without and eliminates other matter; at every moment the cells of its body die away and new cells are formed; after a longer or a shorter time the matter of its body is completely renewed, having been replaced by other atoms, so that each organized being is always the same, and yet different[1]". It is therefore concluded that "life is thus likewise a contradiction present in things and processes, always positing and solving itself, and as soon as the contradiction ceases, life ceases also and death ensues[2]". These conclusions are held by Lysenko and Prezent to exclude the possibility that relatively unchanging elements in organisms can be recognized.

Frequently the objection is made that genetics is in addition dualistic, postulating in living things two disjunct entities, the hereditary substance and the soma, instead of the one universal interacting system recognized by dialectical materialism. This view is shown to be that of Weismann and is strongly condemned. Žebrak[a,b] also joins in the attack on Weismann's dualism but endeavours to dissociate it from modern genetics. Meĭster,[a,b] although joining with such authors as Delaunay in rebutting the more extreme criticisms of Lysenko and Prezent, aligns himself with the latter authors in deprecating the formalism and atomism of Mendelian genetics. Atomism, the particulate concept of the gene, by its attribution to the genes of constant hereditary properties, is regarded by Prezent[c] as likewise inconsistent with dialectics.

Prezent[b] makes a characteristic observation on the genetical discussions between Morgan and Punnet on the genetics of pigmentation in the leghorn fowl. Morgan is stated to have held that white coloration is determined by dominant genes inducing the formation of white individuals, while to Punnet is attributed the belief that white coloration results from dominant genes suppressing colour formation. The whole discussion is ridiculed as a meaningless argument about fictitious entities, scholastically sterile, and typical of the illegitimate mode of abstraction favoured by Mendelian geneticists.

There is little point in recapitulating the theory of Lysenko[j] and Prezent[e] that Mendelian genetics owes many of its alleged defects to its having been nurtured by the bourgeoisie. Charges that certain genetical theories are capitalistic or contra-revolutionary enliven the controversies in the Soviet Union, and have already been dealt with in Chapter III.

More interest attaches to the charge that genetics leads on to racial theories. It is indisputable that modern genetical research has demonstrated the biological inequality of man and that this fact has been utilized by those endeavouring to propagate political theories of racial discrimination. Such theories conflict with Marxian sociology and have been heavily criticized in the Soviet Union. Nevertheless, the notion of social equality and the concept of the classless society cannot be derived from Mendelian principles, since, materially speaking, no such equality exists. This is hardly surprising since it seems that the notion of human equality is derived more from theological than strictly scientific considerations. It may be that the notion of human inequality is inconsistent with Marxian sociology, as Meĭster[b] and Prezent[e] assert; this, however, does not prove that Mendelian theory is thereby false, but merely shows that the former cannot be accepted *a priori* by geneticists, even though it may be conditionally valid in its own realm.

---

[1] ist jedes organische Wesen in jedem Augenblick dasselbe und nicht dasselbe; in jedem Augenblick verarbeitet es von Aussen zugeführte Stoffe und scheidet andre aus; in jedem Augenblick sterben Zellen seines Körpers ab und bilden sich neue; je nach einer längern oder kürzern Zeit ist der Stoff dieses Körpers vollständig erneuert, durch andre Stoffatome ersetzt worden, so dass jedes organisirte Wesen stets dasselbe und doch ein andres ist.

[2] Das Leben ist also ebenfalls ein in den Dingen und Vorgängen selbst vorhandner, sich stets setzender und lösender Widerspruch; und sobald der Widerspruch aufhört, hört auch das Leben auf, der Tod tritt ein.

Attention has already been paid to the distrust shown by Lysenko[p] and Prezent[b] of the use of mathematics in biology. Genetics, they assert, is abiological since it attempts to establish its conclusions by methods improper to the subject. It is charged with exhibiting mathematical formalism and mystic symbolism, and of indulging in algebraic exercises and excursions into statistical theory that are quite outside its realm. The obscurantism of this view has already been shown. Mathematics, which deals with quantitative relations, is a *sine qua non* for the elucidation of the quantitative relations in genetics, and although it is not the key to the whole of genetics, it is essential in the study of its quantitative aspects.

The Pragmatic Test

The practical shortcomings of genetics have been discussed often in recent years, both within and without the Soviet Union. Lysenko and Prezent have undoubtedly won much of the support that they have received in Russia by their claim to be able to further plant breeding objectives in a way impossible to Mendelian genetics. Lysenko[j], Prezent[b,e], Meĭster[b], Šulyndin[c], Eĭhfeljd and Poljakov have all inveighed against the practical uselessness of Mendelism, which they assume to be far inferior in this respect to the system elaborated by Lysenko.

Criticism of genetics on this score is indeed legitimate. The practical defects of genetics have been acknowledged in many countries, and many plant breeders would admit that genetical theory has had little or no direct influence on their practical methods, and has contributed relatively little to their achievements. Several explanations of its lack of practical usefulness have been put forward, including Mather's[b] suggestion that geneticists have paid insufficient attention to the polygenes believed to control physiological characters of economic importance. But lack of practicality does not necessarily discredit the theoretical basis of the science. All geneticists wish, of course, that important practical applications will eventually be derived from genetical theory, but it would be foolish to argue that the present dearth of such applications is anything more than a sign of the immaturity of the science.

On the other hand, it has been shown that there are grounds for questioning whether Lysenko's genetical theories have achieved all the practical results that have been claimed for them. It would be most interesting to discover whether Lysenko's new varieties are grown widely in the Soviet Union, and whether his new theories are proving profitable in breeding practice. Evidence for their practical success is not to hand, and the fact that the original practical measures proposed by Lysenko appear to be largely passed over now that fresh ideas have been mooted, invites the question as to whether the original suggestions did in fact display all the usefulness claimed for them.

Logical Objections

So far the objections raised by Lysenko to Mendelian genetics on *a priori* grounds have alone been considered. In this section the more important objections based on fact and reason will be reviewed.

It will have to be acknowledged straight away that much of Lysenko's criticism is worthless, since it attacks, not the genetics of today, but the genetics of thirty years ago. So many of the arguments brought forward are mere reiterations of Timirjazev's objections to the genetics of the first decade of the century, that they are today totally inappropriate. Timirjazev had a fair grasp of the genetical theories of his time, and was able to point out many of their weaknesses. But since his death, numerous advances have been made in almost every aspect of genetics, and most of his criticisms are by now out of date. Lysenko and Prezent, however, insist on repeating Timirjazev's objections, apparently oblivious of the fact that these criticisms have practically no reference to genetics as it is today. On more than one occasion in this bulletin it has been noted that Lysenko and Prezent exhibit a curious obscurantism in their scientific outlook, and nowhere is this more patent than in their flogging of dead genetical horses.

This concentration on the past is also brought into evidence by the fact that Lysenko and Prezent are apparently unfamiliar with many modern genetical theories. Their remarks betray the fact that they are not informed as to what theories their contemporaries are holding, and their criticisms are often directed to mere travesties of the theories actually supported. It may happen occasionally that the Mendelian theories they consider are distorted only for the purpose of ridicule, a procedure not unknown in the Russian genetical controversies, but whether the distortion is deliberate or not, the Mendelian theories that suffer criticism at the hands of Lysenko and Prezent are seldom the same as those held by competent geneticists today. Many Russian geneticists such as Meĭster,[b] Konstantinov *et al.*, Žebrak, Vavilov and M. M. Zavodovskiĭ have

pointed out this failure of Lysenko to acquaint himself adequately with the world literature, and there seems little doubt that the charge is well founded.

Perhaps the most telling criticism of Mendelian genetics made by any of Lysenko's followers was that made by Prezent[b] in 1936, when he showed that no sharp distinction could be drawn between acquired and inherited characters, or between the genotype and phenotype. Prezent's actual criticism is not very powerful, but he undoubtedly exposes one of the weaker parts of Mendelian theory as generally understood to-day. In spite of Darlington's[b] conclusion that the distinction between hereditary and non-hereditary characters is not absolute, the contrary assumption is frequently encountered in western genetical literature. Yet, in reality, this distinction is only relative, and although convenient in practice in a great many cases, it fails in others. Jollos' experiments on dauermodification, and recent work on virus infection have shown in particular that no fast distinction between the two categories is possible. After all the only observational ground for discriminating is that of duration. If a character persists for a single generation and does not appear in the offspring, it is generally regarded as phenotypic only. If, however, it appears and persists thereafter indefinitely it is regarded as hereditary. But since, as Jollos showed, characters (dauermodifications) could appear which persist for a few, say four or five generations, and then gradually disappear, it is clear that the distinction is one of degree only. Dauermodifications can persist for various lengths of time, and when the length of time exceeds the period of experimental observation, such modifications are termed hereditary, while if the modifications are short-lived and persist as long as or less than a generation, they are regarded as non-hereditary. Hence it is clear that hereditary and non-hereditary characters are but limiting conditions of dauermodification.

Prezent's criticism is justified in as far as geneticists have tended to make the distinction a fast one. The notions of genotype and phenotype retain their value, however, but they must be redefined in relative terms, a rectification which, it is to be hoped, will soon become commonplace in genetical literature.

A criticism, partially justified, but carried to excess, is the attack made by such authors as Lysenko[b,n], Jakubciner[a,b], Prezent, Tatarincev and Silantjev, Meĭster[b], Rjabov, Rodičev, Malahovskiĭ and Razumov[a] on Johannsen's concept of the pure line. If as shown above the distinction between transient and heritable characters is only relative, the pure line concept itself is only capable of a relative significance, and the rejection of the pure line understood absolutely is justified. The attack on the pure line concept by the authors mentioned above, however, goes far beyond this, and is the natural outcome of Lysenko's theory of pure line variability.

Lysenko[c] himself stated that pure lines of the wheat variety Lutescens 062 could be selected for vegetation period, while Vahrušev claimed that segregation occurred in several pure lines of wheat. Vinogradova[a] found that a pure line of the wheat variety Moskovskaja 02411 could be selected to give a yield increase of 24·9%, while Grebennikov claimed that by selecting pure lines of winter wheat, improvement could be obtained in such characters as yield, grain size, frost resistance and resistance to *Puccinia triticina* Erikss. It is obvious that these authors have assumed the truth of Lysenko's theory of pure line variability, which has already been shown to rest on the slenderest foundation. Furthermore, as Sapehin pointed out, the assumption has been made that Russian crop varieties are pure lines, which is very far from the case. Vavilov[a], Žebrak[d], Konstantinov *et al.*, Kozlov and Matveev, and Vakar have all shown that Lysenko's interpretation of the pure line theory is erroneous, and that, moreover, few geneticists maintain that the pure line concept should be applied rigidly. The element of truth in Lysenko's objection lies in the fact that the distinction between hereditary and non-hereditary variation is only relative. The further objection based on Lysenko's theory of pure line variability lacks adequate foundation.

Several critics of Lysenko's theory of pure line degeneration pointed out that in potatoes, at least, the cases of deterioration quoted were probably due to virus infection. This explanation did not commend itself to Lysenko, and he tends to the contrary opinion that many of the plant diseases generally supposed to be caused by virus infections, are in fact merely instances of pure line degeneration.

A partially justified objection was made by Lysenko[b] in 1936, to the effect that Mendelian genetics is too crude a science, its exponents preferring to study the segregation behaviour of gross morphological characters, rather than the finer physiological differences that are of economic importance. This criticism was echoed by Meĭster[b] two years later and was quite relevant at the time. It is, however, not a criticism of genetics as such, but of the choice made by geneticists of subjects to study. It need not be further considered here except to point out that, since then

there has been a marked increase in the number of investigators concerning themselves with the mode of inheritance of physiological characters, though even today a criticism of geneticists on these grounds is not without foundation.

The attack by Lysenko[n] and Prezent[c] on the chromosome theory of genetics and the gene concept has been far less balanced than the preceding objections, and has been principally based on *a priori* arguments concerned with dialectical materialism. These have already been considered, but there does remain the charge made by Prezent that Mendelian genes are merely convenient fictions, which, by suitable mathematical juggling, can be made to fit any observed segregation ratio. The gene concept is, according to Prezent, what has been called in this bulletin an elastic hypothesis, i.e. a theory capable of covering all observed cases irrespective of its truth. This difficulty has also been felt in England, where 'Espinasse[b] has criticized Mather's[a,c] polygene concept on the ground that the number of polygenes are increased arbitrarily until they fit the facts, yet without valid evidence for their real existence. This criticism called forth replies by Darlington[a] and Fisher which only partially met the objection. It can in fact only be met adequately if supplementary evidence is produced for the existence of the genes involved. Linkage experiments and cyto-genetic correlations between chromosomal segments and phenotypic characters may be able to afford additional evidence as to the existence of postulated polygenes, but without these, there is a very real danger that the gene concept may be used as an elastic hypothesis in the way already criticized above.

If the gene concept is used as an elastic hypothesis, Prezent's objection would be sustained, but it is because this notion has been strengthened by so many confirmatory data drawn from cyto-genetical studies that it has been generally accepted.

Lysenko[b] has made a great point of emphasizing that genetical analysis is always made subsequent to crossing and is unable to predict the mode of segregation beforehand. He remarks that "from the standpoint of Morganism one can explain anything—but it is impossible to foresee anything with certainty[1]". That dominance relations are unpredictable in the present state of genetical research is true, and similarly for the segregation pattern expected for any particular character. When however genetical analysis has been made of any character in one parental combination, it is frequently possible to predict its behaviour in other combinations. Also, it is well known that mutants tend to be recessive to the wild type alleles. Yet Lysenko's main contention is true, and must be admitted as indicating a deficiency in our present knowledge.

But why this shortcoming should be regarded as demonstrating the falsity of genetics is not clear. No science can claim to have satisfactorily explained all its data, and genetics is one of the youngest sciences. Both Lysenko and Prezent claim that dominance relations can be predicted by their theory, but this claim has already been shown to rest on insufficient evidence. No further reply is needed to this objection, except to admit the present dearth of knowledge on the physiology of dominance, a matter which only further experimentation can be expected to elucidate.

This last criticism brings up a host of objections made by Lysenko and his followers to Mendel's laws. Both Lysenko[n,p,t] and Prezent have repeatedly stated that F_2 segregations frequently diverge from the 3 : 1 ratio postulated by Mendelism, apparently without realizing that many characters are regarded as polymerically determined. Ermolaeva[a] performed experiments with peas which, she asserted, disproved Mendel's conclusions, but her results were later shown by Kolmogorov[a] to lack significance, a finding which increased Lysenko's antipathy to mathematics. Hačaturov[b] also claimed to have obtained non-Mendelian segration in peas. In addition he made two intervarietal wheat crosses, Azerbaijan 17019 x Red Fife, and Azerbaijan 17019 x Garnet, and claimed that though the F_2 families obtained from the F_1 plants were fairly uniform *inter se*, they differed considerably from one another in such characters as vegetative vigour, tillering capacity, height, and number, size and colour of leaves. These results were urged against Mendel's laws along with many others.

The behaviour of Lysenko in arranging experiments to disprove the primitive Mendelian theory of Timirjazev's time is remarkable. The whole notion of polymeric genetical determination is usually ignored, also the fact, which Haldane[b] pointed out to him, that deviations from 3:1 ratios even with monomerically determined characters, are to be expected on statistical grounds. All the work intended to disprove the universality of F_2 3:1 segregation is vitiated through lack of realization of the statistical basis of this generalization.

Practically all the experiments performed by Lysenko and his followers in support of their

[1] С позиций морганизма об'яснить можно все—только нельзя ничего уверенно предвидеть.

theory have also been urged against Mendelism. The degeneration of pure lines is claimed as contradictory to the notion of gene stability, while rejuvenation and heterosis are asserted to be inexplicable on Mendelian principles. The experiments quoted on environmentally modified segregation are brought forward as disproving Mendel's law of independent assortment. Experiments on Millardetism are believed to disprove Mendel's segregation laws, and a similar use is made of the reported instances of F_1 heterogeneity and differences between reciprocal crosses. Internal genetical variation, it is stated, has no place in Mendelism, nor mixed inheritance. The whole notion of environmentally induced variation is held to be contradictory to the rigid formalism of modern genetics, and the facts of selective fertilization and vegetative hybridization are brought forward as final nails in the Mendelian coffin.

It will be obvious that these charges, based as they are in many cases on experimental researches of dubious value, display very little of the spirit of co-operative research. Many of the facts brought forward by Lysenko as contradictory to modern genetics have actually long been comfortably accommodated therein. Differences in reciprocal hybrids, some cases of internal genetic variation, and several cases of mixed inheritance have long been known, and modern research is tending to accept the possibility of some forms of directed mutation.

Perhaps the only experimental results of Lysenko worth considering, which appear inconsistent at first sight with modern genetics are his experiments on grafting. As yet these have not been confirmed, and until this is done, speculation on their interpretation is hardly profitable. None the less, even were they confirmed, there is no reason to suppose that modern genetical theory, which is a growing structure and not the narrow dogmatism imagined by Lysenko and Prezent, will be unable to develop its tenets further if such proved to be necessary.

Lysenko and Prezent have repeatedly brought forward many other of the experiments reviewed in Chapter IV as disproving Mendelism. Arnautov regards the evidence for pure line variability as destroying Mendelism; Vinogradova[b] and Drobinskiĭ quote departures from 3:1 F_2 segregations with the same intention; Tartarincev and Silantjev declare that heterogeneous F_1 generations and the evidence for selective fertilization are similarly indicative; while Šulyndin[a] quotes reciprocal differences in wheat hybrids as contrary to the principles of Morgan-Mendelian genetics.

Lysenko's objection that other geneticists have refused to repeat his experiments remains, however, justified. He himself is, of course, open to the same charge, and his attitude has been so violently antagonistic to international genetics, that the resentful attitude of Mendelian geneticists is not surprising. It seems clear that little further progress can be made until a more accommodating attitude is adopted both by Lysenko and the international school of genetics. Some of Lysenko's results are certainly suggestive, but for the reasons already analysed they cannot be accepted without confirmation. If the industry displayed by members of both sides of the controversy could be expended in co-operative research, considerable progress might be made in such problems as heterosis and grafting phenomena. One of the principal objects of this bulletin has been to make possible such an approach to the subject. An attempt has been made to present all the evidence as it appears in the published writings of Lysenko and his school, and to analyse both the merits and the defects of his arguments. It is earnestly hoped that by so doing, much misunderstanding will have been removed and geneticists of each school will be encouraged to examine their own and each others data in an unprejudiced light. This should lead ultimately to a synthesis of what is best in both schools, thereby achieving that comprehensive understanding of genetical questions which is the aim of both bodies of investigators.

SUMMARY

The school of genetics founded by Lysenko and Prezent in the Soviet Union, arose in 1935 and became dominant in Russia in 1940. It still flourishes, although perhaps less now than formerly.

Its characteristic tenets are derived from the writings of Darwin, Timirjazev, Mičurin and Burbank, whose theories have been synthesized with the philosophy of dialectical materialism. In contrast to western procedure, much of the scientific discourse of Lysenko's school is alogical, i.e. derives its conclusions not by logical argument from the facts, but by appeal to chosen authorities, by condemning views in opposition to these authorities, by analysing the presumed states of mind of its opponents, and by estimating the value of theories by their agronomic usefulness.

The facts reported by Lysenko in support of his system are as follows:—

(1) An F_1 hybrid cannot be later than its earlier parent.
(2) Biologically advantageous characters are dominant.
(3) F_2 transgressive segregation for earliness is impossible.
(4) Pure lines necessarily degenerate on selfing.
(5) Intravarietal crossing gives rise to increased vegetative vigour.
(6) Genetic variation is induced by environmental factors.
(7) Segregation ratios are determined by the environment.
(8) F_2 hybrid progenies may fail to segregate.
(9) The F_1 generation of homozygous parents may be heterogeneous.
(10) Reciprocal hybrids may differ *inter se.*
(11) Different parts of the same individual may differ genetically.
(12) F_1 hybrids may exhibit a mosaic of tissues derived from either parent respectively.
(13) Grafting may bring about genetic interaction between stock and scion.

Many of these generalizations are stated by Lysenko and Prezent to hold only under appropriate but unspecified environmental conditions. Much of the evidence for them is inconclusive.

The hypothesis advanced by Lysenko to cover these presumed facts is the nutrient theory. Organisms are regarded, not as enduring entities, but as streams of stages, each stage arising from the preceding through absorption (assimilation), by the preceding stage, of environmental elements (nutrients) to give a new compound, the next stage in development. Each generation is regarded as a cycle of developmental stages. The similarity between parents and offspring is attributed to conservatism, a property of all living matter. Assimilation of nutrients is not at random but selective, organisms having the power to select nutrients which will combine with the organism to produce a biologically advantageous course of development. Selective fertilization, the selection by the ovum of biologically advantageous pollen grains, here acting as nutrients, is a special manifestation of the organism's general capacity for selective assimilation. If the nutrients are not of like nature to the selecting organism its conservatism is overcome and new cycles of development are initiated. In this way the environment can modify the nature of organisms. Local differences between nutrients within an individual plant result in internal genetic variation. When this occurs in the sporogenous tissue, differentation of gametes and subsequent segregation occur. Hybridization is regarded as an additive process, leading to the combination in one plant of two developmental potentialities, while segregation is conceived as a subtractive process dispersing developmental potentialities. Crossing is supposed to result in hybrid vigour through the combination of dialectically opposite gametes; selfing is believed to be deleterious through the absence of dialectically opposite gametes. Biologically advantageous characters are believed to be dominant, the organism selecting those nutrients which will combine with it to initiate the most favourable cycle of development.

These theories, although exhibiting a certain degree of internal coherence, contain various inconsistencies and receive only slight support from the facts. Lysenko's rejection of the data accumulated by Mendelian genetics during the past thirty years is obscurantist and reduces the value of his speculations.

Mendelian genetics is criticized by Lysenko for its failure to conform to his chosen authorities, for its claimed inconsistency with dialectical materialism, and for the supposed discrepancies between its tenets and Lysenko's experimental results.

SOMMAIRE

L'école génétique de Lysenko et Prezent a été établie en 1935 dans l'U.R.S.S., et devint dominante dans l'Union Soviétique en 1940. Elle prospère encore aujourd'hui, quoiqu'il semble que son influence soit moins prononcée.

Ses principaes caractéristiques dérivent des ouvrages de Darwin, de Timirjazev, de Mičurin et de Burbank, dont les théories ont été synthetisées avec la philosophie du matérialisme dialectique. En contraste avec le procédé de l'occident, une grande partie du discours scientifique de l'école de Lysenko est alogique, à savoir, elle ne dérive pas ses conclusions des faits par l'argument logique, mais elle discute par l'appel aux autorités, par la condamnation des opinions en opposition avec ces autorités, par l'analyse des sentiments supposés de ses antagonistes, et par l'apréciation de la valeur des théories selon leur utilité agronomique.

Lysenko constate les faits suivants à l'appui de son système:—

(1) Un hybride F_1 ne peut pas être plus tardif que le parent plus précoce.
(2) Les caractères biologiquement avantageux sont dominants.
(3) La disjonction transgressive quant au caractère de précocité dans la génération F_2 est impossible.
(4) Les lignes pures soumises à l'autofécondation subissent nécessairement la dégénération.
(5) L'hybridation intravariétale effectue une augmentation de la vigueur végétative.
(6) Les facteurs du milieu peuvent causer la variation génétique.
(7) Le milieu détermine les proportions des génotypes dans la génération hybride F_2.
(8) Les familles hybrides F_2 ne disjoignent pas toujours.
(9) La génération F_1 des parents homozygotes peut être hétérogène.
(10) Les hybrides réciproques peuvent différer l'un de l'autre.
(11) Les diverses parties d'un individu peuvent différer génétiquement.
(12) Les hybrides peuvent manifester une mosaïque de tissus dérivés des deux parents.
(13) Le greffage peut causer l'intéraction entre le scion et le porte-greffe.

Lysenko et Prezent tiennent que beaucoup de ses généralisations sont valides seulement selon certaines conditions convenables, mais qui cependant ne sont pas précisées. Une grande partie de l'évidence appuyant ces généralisations n'est pas décisive.

L'hypothèse introduite par Lysenko pour expliquer ces faits présumés est la théorie de la nourriture. Lysenko ne regarde pas les organismes comme des entités durantes, mais il considère chacun comme un flux de phases, dont chacune naît de la phase précédente au moyen de l'absorption (assimilation) par cette phase précédente d'éléments (nourriture) du milieu pour engendrer un composé nouveau, à savoir, la phase suivante de développement. Chaque génération pour Lysenko est un cycle de phases ontogénétiques. Il constate que la ressemblance entre les parents et leurs descendants est la conséquence du conservatisme, propriété générale de toute matière vivante. L'assimilation de nourriture ne se réalise pas au hasard mais sélectivement, les organismes ayant la capacité de choisir telle nourriture qui s'amalgame avec lui pour produire un cours ontogénétique biologiquement avantageux. La fécondation sélective, c'est à dire, le choix du coté de l'œuf des graines de pollen biologiquement avantageuses, celles-ci jouant le rôle de nourriture, est une manifestation spéciale de la capacité générale des organismes pour l'assimilation sélective. Si la nature de la nourriture diffère de celle de l'organisme choisissant, le conservatisme de celui-ci est liquidé et les nouveaux cours ontogénétiques commencent. Ainsi le milieu peut modifier la nature des organismes. Les différences locales chez l'individu végétal à l'égard de la nourriture engendrent la variation génétique interne. Quand cette variation interne se trouve dans le tissu sporogène, les gamètes se différentient et la disjonction s'effectue. Lysenko regarde l'hybridation comme procédé additif engendrant dans une plante, la coalition de deux potentialités ontogénétiques; quant à la disjonction, il l'imagine comme procédé subtractif qui disperse les potentialités ontogénétiques. Selon Lysenko, l'hybridation engendre la vigueur hybride par la fusion de gamètes dialectiquement opposés; l'autofécondation est délétère par suite du manque de gamètes dialectiquement opposés. Les caractères biologiquement avantageux sont dominants à cause du choix par l'organisme de cette nourriture qui se combinant avec lui initie le cours ontogénétique le plus favorable.

Ces théories, bien que montrant une certaine cohérence intrinsèque, révèlent aussi diverses contradictions, et elles sont très peu appuyées par les faits.

Le rejet par Lysenko des faits amassés par la génétique mendélienne pendant trente ans est obscurantiste, et il réduit la valeur de ses spéculations.

Lysenko censure la génétique mendélienne à cause de sa contradiction avec ses autorités, de son opposition putative avec le matérialisme dialectique, et des incompatibilités supposées entre le mendélisme et ses expériences pratiques.

RESÚMEN

La escuela de genética fundada por Lysenko y Prezent se creó en el año 1935, y su influencia ha sido dominante en la Unión Sovietica desde 1940. Florece todavía, pero posiblemente algo menos que antes.

Sus puntos de vista mas característicos se derivan de las obras de Darwin, Timirjazev, Mičurin y Burbank, cuyas teorías han sido sintetizadas con la filosofía del materialismo dialéctico.

En contraste con el procedimiento aceptado en occidente, una gran parte del discurso científico de la escuela de Lysenko es alógico, es decir, no deduce sus conclusiones de los hechos por argumento lógico, sino se refiere a las autoridades escogidas, analiza los presuntos estados mentales de sus oponentes y estima el valor de las teorías según su eficacia agronómica.

Las observaciones que aduce Lysenko en apoyo de su sistema son:—

(1) Un híbrido F_1 no puede ser más tardío que el progenitor mas precoz.
(2) Los caracteres que son biologicamente ventajosos son dominantes en la herencia.
(3) La transgresión por precocidad es imposible.
(4) Las lineas puras degeneran necesariamente al autofecundarse.
(5) Cruzamientos intravarietales producen un aumento de vigor vegetal.
(6) La variación genética se induce por los factores del ambiente.
(7) El ambiente determina las proporciones de los tipos en la segunda generación.
(8) La segregación puede estar ausente en las generaciones de F_2.
(9) La primera generación obtenida por cruzamiento de plantas homozigotas puede ser heterogénea.
(10) Los híbridos recíprocos pueden ser distintos.
(11) Las diferentes partes del mismo indivíduo pueden ser genéticamente distintas.
(12) Los híbridos F_1 pueden manifestar un mosaico de tejidos que provienen de ambos progenitores.
(13) El injerto puede ocasionar una interacción genética entre el porta-injerto y el injerto.

Una gran parte de estas generalizaciones valen, según Lysenko y Prezent, unicamente en condiciones apropiadas, las cuales, sin embargo, los autores no precisan. La evidencia para las generalizaciones es en gran parte inconclusiva.

La hipótesis propuesta por Lysenko para explicar estos presuntos hechos es la teoría nutritiva. Los organismos se consideran no como entidades de duración sino como corrientes de etapas sucesivas, cada una de las cuales proviene de la etapa precedente por medio de la absorción (asimilación) de los elementos ambientales (alimentos), con la producción de una substancia nueva, la siguiente etapa del desarrollo. Cada generación se trata como ciclo de etapas de desarrollo. La semejanza entre los padres y hijos es atribuida al conservatismo, una propiedad de toda materia viva. La asimilación de los alimentos no se realiza al azar sino selectivamente, pues los organismos poseen la capacidad de elegir los alimentos que se combinen con el organismo de tal manera que se produzca una corriente de desarrollo que sea biologicamente ventajosa. La fecundación selectiva, es decir la selección por el huevo de los granos de polen más ventajosos biologicamente, representa una manifestación especial de la capacidad de asimilación selectiva del organismo. Si el alimento es de naturaleza distinta de la del organismo que elige, se vence el conservatismo del mismo y nuevos ciclos de desarrollo se inician. Así, el ambiente puede modificar la naturaleza de los organismos. Las diferencias de una parte de la planta a otra en lo que concierne a los alimentos producen una variación genética interna. Cuando eso tiene lugar en el tejido esporógeno, resulta una diferenciación de los gametas y una consecuente segregación. Se trata la hibridación como un proceso aditivo que conduce a la combinación en una sola planta da dos potencialidades de desarrollo. La segregación está concebida como un proceso substractivo, que reparte a las potencialidades de desarrollo. Se considera que el aumento de vigor que resulta del cruzamiento proviene de la combinación de gametas que son dialecticamente opuestos; la autofecundación tiene un efecto nocivo, se cree, por causa de la falta de los gametas dialecticamente opuestos. Se cree que los caracteres biologicamente ventajosos son dominantes, y que el organismo elige los alimentos que se combinan con el para iniciar el ciclo de desarrollo más favorable.

Estas teorías, aunque demuestren un cierto grado de conformidad interna contienen algunos puntos de inconsistencia y reciben poco apoyo de los hechos. El repudio por Lysenko de todos los datos acumulados por la genética mendeliana durante los ultimos treinta años es obscurantista y disminuye el valor de sus especulaciones.

Lysenko critica a la genética mendeliana por el hecho de no conformar con sus autoridades, de ser inconsistente con el materialismo dialéctico y de estar en contradicción con sus resultados experimentales.

ZUSAMMENFASSUNG

Die von Lysenko und Prezent gegründete Vererbungslehreschule ist im Jahre 1935 entstanden und ab 1940 vorherrschend geworden. Der Einfluss der Schule bleibt zwar stark, scheint aber heute etwas geringer als früher zu sein.

Ihre eigentümliche Gesichtspunkte stammen von den Arbeiten von Darwin, Timirjazev, Mičurin und Burbank her, deren Theorien mit der Philosophie vom dialektischen Materialismus verschmolzen worden sind. Ein grosser Teil der wissenschaftlichen Beweisführung der Lysenkoschen Schule ist alogisch, womit sie im stärksten Gegensatz zu dem westlichen Gedankengang steht. Die Schussfolgerungen werden nämlich nicht durch logischen Zusammenhang von den Tatsachen gezogen, sondern dadurch, dass Lysenko die von ihm erwählten Autoritäten heranzieht, eine Stellung gegen alle in Widerspruch mit diesen Autoritäten stehenden Ansichten einnimmt, den vermuteten Geisteszustand der Gegner analysiert und den Wert einer Theorie nach ihrer agronomischen Brauchbarkeit misst.

Die Tatsachen, die Lysenko zur Stütze seines Systems beibringt lauten:—

(1) Ein F_1 Bastard könne nicht später als der Frühreifste seiner eltern reifen.
(2) Die biologisch günstigen Merkmale dominieren.
(3) Transgressionsspaltung nach Frühreife im F_2 sei nicht möglich.
(4) Die Spaltung reiner Linien sei unvermeidlich.
(5) Kreuzung innerhalb einer Sorte rufe eine gesteigerte Wüchsigkeit hervor.
(6) Genetische Variation werde durch die Umgebungsfaktoren induziert.
(7) Die Spaltungsverhältnisse seien von der Umgebung bedingt.
(8) In der F_2-Nachkommenschaft könne eine Spaltung ausbleiben.
(9) Die F_1-Nachkommenschaft von homozygoten Eltern könne heterogen sein.
(10) Die reziproken Nachkommenschaften können sich voneinander unterscheiden.
(11) Die verschiedenen Teile eines einzigen Individuums können genetisch verschieden sein.
(12) Die F_1-Nachkommenschaften können ein Mosaik aus von beiden Eltern herstammenden Geweben darstellen.
(13) Die Pfropfung könne einen gegenseitigen Einfluss zwischen Pfropf und Reis ausüben.

Diese Verallgemeinerungen sollen hauptsächlich nur unter geeigneten doch von Lysenko und Prezent nicht genau bestimmten Bedingungen gültig sein. Die Beweise für diese Verallgemeinerungen sind meistens nicht überzeugend.

Die von Lysenko zur Erklärung dieser Tatsachen angeführte Hypothese ist die Nährstofftheorie. Die Organismen werden nicht als Dauerwesen betrachtet sondern als Phasenströme; jede Phase entwickelt sich von der vorhergehenden Phase durch Aufnahme (Assimilation) von Umgebungselementen (Nährstoffen) wodurch eine neue Verbindung, und zwar die nachfolgende Entwicklungsphase, entsteht. Jede Generation wird als ein Entwicklungsphasenkreislauf angesehen. Die Ähnlichkeit zwischen Eltern und Nachkommen wird dem Konservatismus, einer Eigenschaft aller lebendigen Materie, zugeschrieben. Die Assimilation von Nährstoffen ist nicht zufällig sondern selektiv, da die Organismen die Fähigkeit besitzen, solche Nährstoffe auszuwählen, die sich mit dem Organismus so verbinden, dass ein biologisch bevorzugter Entwicklungslauf einsetzt. Ein besonderer Fall der Fähigkeit des Organismus für selektive Assimilation ist die selektive Befruchtung. Diese bestehe darin, dass die Eizelle die biologisch güngstigsten Pollenkerne, die hier als Nährstoffe gelten, auswählt. Wenn die Nährstoffe von anderer Natur sind als die des Organismus, der sie auswählt, so ist sein Konservatismus überwunden und neue Entwicklungskreisläufe setzen ein. In dieser Weise wird die Natur der Organismen von der Umgebung modifiziert. Lokale Unterschiede in den Nährstoffen innerhalb einer einzelnen Pflanze rufen eine innere Variation hervor. Wenn diese im sporogenen Gewebe vorkommen, so entsteht eine Differenzierung der Gameten, die zu einer Spaltung führt. Bastardierung wird als ein additiver Vorgang betrachtet, der zur Verbindung der beiden Entwicklungsfähigkeiten in einer Pflanze führt. Die Spaltung soll dagegen einen subtrahierenden Vorgang darstellen, der die Entwicklungsfähigkeiten verteilt. Die Kreuzung soll eine Bastardwüchsigkeit zustandebringen, die aus der Verbindung dialektisch entgegengesetzter Gameten entsteht; die Selbstbefruchtung wird für schädlich gehalten, da die dialektisch entgegengesetzten Gameten fehlen. Die biologisch günstigen Eigenschaften seien dominant, indem der Organismus nur solche Nährstoffe auswählt, die sich mit ihm so verbinden, dass der günstigste Entwicklungskreislauf einsetzt.

Diese Theorien beweisen zwar einen gewissen inneren Zusammenhang, enthalten aber doch einige Widersprüche und stimmen nur teilweise mit den Tatsachen überein. Lysenkos völlige Ablehnung der vielen Daten, die von der Mendelschen Vererbungslehre in den letzen 30 Jahren gesammelt worden sind, ist obskurantistisch und beeinträchtigt den Wert seiner Spekulationen.

Der Mendelschen Vererbungslehre wirft Lysenko folgendes vor; sie stimme mit seinen Autoritäten, mit dem dialektischen Materialismus und mit seinen Versuchsergebnissen nicht überein.

ОБЩИЕ ВЫВОДЫ

Генетическая школа, основанная Лысенко и Презентом в Советском Союзе, возникла в 1935 г. и к 1940 г. стала ведущей в СССР. Она все еще процветает и теперь, хоть и в меньшей степени, чем в прошлом.

Ее характерные положения имеют своим источником труды Дарвина, Тимирязева, Мичурина и Бербанка, теории которых были спаяны с философией диалектического материализма. В отличие от принятого на Западе подхода многое из научного обоснования школы Лысенко является алогичным. Иначе говоря, заключения делаются не как результат исходящей из наличных фактов аргументации, а путем ссылки на избранные авторитеты, путем осуждения противоречащих этим авторитетам взглядов, путем анализа предполагаемого настроения умов противников и, наконец, путем примерной оценки теорий согласно их агрономической полезности.

Лысенко приводит в доказательство своей системы следующие факты:

(1) Гибрид F_1 не может быть более позднеспелым, чем его скороспелый родитель.
(2) Биологически выгодные признаки являются доминантными.
(3) Нельзя получить трансгрессивное расщепление признака скороспелости в гибриде F_2.
(4) Чистые линии обычно вырождаются при самоопылении.
(5) Внутрисортовое скрещивание ведет к увеличенной мощности вегетативного развития.
(6) Генетическая изменчивость возникает под влиянием внешней среды.
(7) Цифровые пропорции расщепления определяются внешней средой.
(8) В потомстве гибрида F_2 может отсутствовать расщепление.
(9) Поколение F_1 гомозиготных родителей может быть неоднородным.
(10) Гибриды обратного скрещивания могут отличаться между собой.
(11) Разные части одной и той же особи могут быть генетически отличны.
(12) Гибриды F_1 могут представлять собой мозаику тканей, происходящих соответственно от обоих родителей.
(13) Прививка может привести к взаимодействию между подвоем и привоем.

Многие из этих обобщений остаются в силе, согласно указаниям Лысенко и Презента, только в подходящих, но точно не указанных внешних условиях. Многое из приведенных доказательств является неубедительным.

Гипотеза, выдвинутая Лысенко для объяснения этих предполагаемых фактов, основывается на теории питания. Организмы рассматриваются не как неизменные самодовлеющие единицы, а как течение стадий, при чем каждая последующая стадия возникает из предшествующей путем усвоения ею элементов (пищи) из окружающей среды, что и дает новый комплекс—последующую стадию развития. Каждое поколение рассматривается как цикл стадий развития. Сходство между родителями и потомством приписывается консерватизму—качеству, присущему всей живой материи. Усвоение питательных веществ происходит не случайно, а избирательно. Организмы в состоянии выбирать питательные вещества, которые; будучи связаны с организмом, и вызывают биологически выгодный путь развития. Избирательное оплодотворение—выбор яйцом биологически выгодных пыльцевых зерен, действующих как питательное вещество в этом случае—является частным случаем проявления общей способности организма к избирательной ассимиляции. Если питательные вещества неодинаковы по своей природе с обладающим избирательной способностью организмом, его консерватизм преодолевается и, таким образом, возникают новые циклы развития. Этим путем окружающая среда может видоизменять природу организмов. Местные различия между питательными веществами в самой растительной особи ведут к внутренней генетической изменчивости. Когда это происходит в споро-образующих тканях, в конечном итоге выявляется дифференцировка половых клеток (гамет) с последующим расщеплением. Гибридизация рассматривается как наслоительно-слагающий процесс, ведущий к сочетанию в одном растении двух возможных путей развития. Расщепление же считается уменьшающе-вычитательным процессом, рассеивающим возможности путей развития. Предполагается, что скрещивание ведет к проявлению мощности в гибридах вследствие сочетания диалектически

противоположных гамет, а самоопыление считается вредным из-за отсутствия диалектически противоложных гамет. Биологически выгодные признаки рассматриваются как доминантные, ибо организм избирает как раз такие питательные вещества, которые, будучи им усвоены, дадут начало самому благоприятному циклу развития.

Невзирая на то, что эти теории показывают некоторую меру внутренней связности, они все же содержат целый ряд противоречий и только в слабой степени подтверждаются фактами. Лысенко, отвергая накопленные в течение последних 30 лет данные менделевской генетики, тем самым воздает дань мракобесию и низводит ценность своих соображений.

Лысенко критикует менделевскую генетику за недопустимое упущение—расхождение с избранными авторитетами, за воображаемое противоречие с диалектическим материализмом, а также из-за предполагаемых расхождений между ее положениями и его собственными опытными результатами.

BIBLIOGRAPHY

ALPATJEV, A. V. (Intravarietal crossing in tomatoes). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1941: No. 1: 62–69. [P.B.A.,* XIV, 346].

a ANANJEVA, S. V. (Vegetative hybridization of sunflower). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 4: 168–70. [P.B.A., XII, 223].

b —— (The mentor method applied to the sunflower). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1941: No. 2: 118–33. [P.B.A., XII, 555].

c —— (The vegetative hybridization of sunflowers). *Bjulletenj Instituta Zernovogo Hozjaĭstva Jugo-Vostoka S.S.S.R.* (*Bull. Inst. Grain Husb. S.E. U.S.S.R.*) *Saratov* 1944: No. 2: 19–25. [P.B.A., XVI, 410].

a ANONYMOUS. (Fourth session of the All-Union Lenin Academy of Agricultural Sciences). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: No. 2: 2–47. [P.B.A., VIII, 65].

b —— (Achievements and prospects of Soviet linen flax breeding). *Len i Konoplja* (*Flax and Hemp*) 1940: No. 10: 22–26. [P.B.A., XIII, 233].

c —— Genetics in the Soviet Union. Three speeches from the 1939 Conference on Genetics and Selection. *Sci. and Soc.* 1940: **4**: 183–233. [P.B.A., XI, 321].

d —— (Six years' work of the State Breeding Stations). *Socialističeskoe Seljskoe Hozjaĭstvo* (*Socialistic Agriculture*) *Moscow* 1943: No. 8: 42–50. [P.B.A. XIV, 1119].

ARNAUTOV, V. V. (Transmutation of the potato plant by controlled nutrition). *Ovoščevodstvo* (*Vegetable Growing*) 1939: No. 12: 24–26. [P.B.A. X, 784].

AVAKJAN, A. A. and JASTREB, N. G. (Hybridization by means of grafts). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): 50–77. [P.B.A., XII, 82].

AVERJJANOVA, O. P. (Intravarietal crossing in the egg-plant). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): 106–08. [P.B.A., XII, 284].

BASSARSKAJA, M. A., ERMOLAEVA, N. I. and HODJKOV, L. A. (Description of wheat plants obtained from seed of intravarietal crosses). *Jarovizacija* (*Vernalization*) 1940: No. 1 (28): 23–26. [P.B.A., XI, 348].

BERDYAEV, N. A. *The origin of Russian Communism.* Trans. R. M. French, London 1937: Pp. 239.

BERLJAND, S. S. (The inheritance by the potato of aerial tuber formation as a result of grafting). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 107–09. [P.B.A., XII, 1104].

a BERNAL, J. D. Dialectical Materialism. *Aspects of Dialectical Materialism,* London 1934: 89–122.

b —— *The Social Function of Science.* London 1939: Pp. 482.

BOLDYREV, A. N. (The question of changing the nature of plants by grafting). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1940: No. 6: 5–7. [P.B.A., XI, 832].

BOLSUNOV, I. Zur Untersuchung der Heterosis bei *Nicotiana rustica* L. V. Fortgesetzte Selbstbefruchtung und Ernteertrag. *Z. Pflanzenz.* 1944: **26**: 223–44. [P.B.A., XV, 1463].

a BREŽNEV, D. D. (Intravarietal crossing as a method of increasing yield in tomatoes). *Ovoščevodstvo* (*Vegetable Growing*) 1939: No. 6: 38–41. [P.B.A., IX, 1646].

b —— (The capacity for selective fertilization in tomatoes). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1939: No. 5: 14–15. [P.B.A., IX, 1379].

c —— (Selective fertilization and urgent tasks of breeding and seed growing). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1941: No. 1: 20–23.

BURBANK, L. *An Architect of Nature.* London 1939: Pp. 139.

a CÂMARA, A. DE SOUZA DA. Genética e selecção. *Rev. Agron. Lisboa* 1939: **27**: 410–21. [P.B.A., XII, 390].

b —— A genética continua a ser atacada. *Rev. Agron. Lisboa* 1940: **28**: 330–32. [P.B.A., XII, 390].

CATCHESIDE, D. G. Polarized segregation in an ascomycete. *Ann. Bot. Lond.* 1944: **8**: 119–30. [P.B.A., XV, 16].

CINDA, K. I. (Intravarietal crossing in cotton). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 51–55. [P.B.A., XII, 1115].

a DANIEL, L. Hérédité de la propriété remontante chez les plantes greffées. *C.R. Acad. Sci. Paris* 1926: **182**: p. 84.

b —— Nouvelles recherches sur l'hérédité acquise par greffe chez l'*Helianthus Dangeardi*. *C.R. Acad. Sci. Paris* 1926: **182**: 800–01.

c —— Nouveaux hybrides de greffe. *Rev. Hort. Paris* 1936: **108**: 186–90. [P.B.A., VII, 107].

d —— Les *Crataegomespilus*. *Rev. Hort. Paris* 1937: **109**: 446–49. [P.B.A., VII, 1172].

a DARLINGTON, C.D. The polygene concept. *Nature, Lond.* 1942: **150**: p. 154. [P.B.A., XII, 905].

b —— Heredity, development and infection. *Nature, Lond.* 1944: **154**: 164–69. [P.B.A., XV, 17].

a DARWIN, C. *The Origin of Species.* London 1878: 6th Ed.: Pp. 458.

b —— *Animals and Plants under Domestication. London* 1890: 2nd Ed.: Pp. 473+495.

c —— *The Effects of Cross and Self Fertilisation of Plants.* London 1900: 2nd Ed.: Pp. 487.

DELAUNAY, L. (Does "formal genetics" give anything of practical use for the production of new varieties). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 59–68. [P.B.A., VIII, 736].

DELONE, *see* DELAUNAY.

DMITRIEV, K. M. (Under the banner of Darwinism. At the State Conference on Breeding and Seed Production). *Jarovizacija* (*Vernalization*) 1939: No. 2 (23): 117–22.

a DOLGUŠIN, D. A. (Intravarietal crossing). *Novoe v Seljskom Hozjaĭstve* (*What is new in Agriculture*) *Moscow* 1937: No. 11: Pp. 30. [P.B.A., VIII, 110].

b —— (The seed production work of the breeding and genetics institute). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1939: No. 5: 16–17. [P.B.A., IX, 1437].

c —— (Seed production of grain crops). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): 6–26. [P.B.A., XII, 118].

* Plant Breeding Abstracts.

Drobinskiĭ, B. N. (Segregation of the F_2 generation of grey mustard as indicated by colour of seed and Mendel's "law"). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 117–18. [P.B.A., XII, 1152].

Dvorjankin, F. (Summing up). *Jarovizacija* (*Vernalization*) 1940: No. 5 (32): 23–34. [P.B.A., XI, 903].

Eichfeld, *see* Eĭhfeljd.

Eĭhfeljd, I. G. (The establishment of food-production in the far north). *Doklady Vsesojuz. Akad. Selsjk. Nauk im. V.I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*). 1944: No. 1: 3–8. [P.B.A., XV, 875].

Emerson, S. The induction of mutations by antibodies. *Proc. Nat. Acad. Sci. Wash.* 1944: **30**: 179–82. [P.B.A., XV, 116].

a Engels, F. *Herrn Eugen Dühring's Umwälzung der Wissenschaft.* Stuttgart 1901: Pp. 354.

b —— *Dialectics of Nature.* Trans. C. Dutt. London 1940: Pp. 383.

a Ermolaeva, N. I. (Once more on the "pea laws"). *Jarovizacija* (*Vernalization*) 1939: No. 2 (23): 79–86.

b —— (An experiment in vegetative hybridization in order to obtain economically valuable types of tomato). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 31–34. [P.B.A., XII, 1221].

a 'Espinasse, P. G. Genetics in the U.S.S.R. *Nature, Lond.* 1941: **148**: 739–43. [P.B.A., XII, 308].

b —— The polygene concept. *Nature, Lond.* 1942: **149**: 731–32. [P.B.A., XII, 904].

Fersman, A. Science in the U.S.S.R. *Advanc. Sci.* 1944: **3**: No. 9: 62–77. [P.B.A., XV, 1].

a Filippov, A. (On vegetative hybridization of the potato). *Plodoovoščnoe Hozjaĭstvo* (*Fruit and Vegetable Growing*) 1938: No. 11: 26–31. [P.B.A., IX, 769].

b —— (Interspecific and intraspecific vegetative hybridization of potatoes). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 2: 16–39. [P.B.A., XIII, 1276].

c —— (Mixed inheritance and vegetative segregation in the potato). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 5: 106–12. [P.B.A., XIV, 235].

d —— ("Vegetative rapprochement" as a means of overcoming cross-incompatibility in potatoes). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1941: No. 1: 36–42. [P.B.A., XIV, 234].

Fisher, R. A. The polygene concept. *Nature, Lond.* 1942: **150**: p. 154. [P.B.A., XII, 905].

Fomin, P. (Vegetative hybridization in legumes). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 4: 170–71. [P.B.A., XII, 286].

Gaškova, A. (Grafting as a means of plant transformation). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V.I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: Nos 8–9: 12–18. [P.B.A., XV, 1192].

Gilson, E. *The Unity of Philosophical Experience.* London 1938: Pp. 340.

a Gluščenko, I. E. (Genotypical variation of the potato resulting from conditions of growth). *Jarovizacija* (*Vernalization*) 1940: No. 6 (33): 98–101. [P.B.A., XII, 176].

b —— (Results of twice repeated cross-pollination of rye in the plots of the collection). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): 27–33. [P.B.A., XII, 137].

c Gluščenko, I. E. (The retention of typical varietal characters of buckwheat after intervarietal pollination). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 68–75. [P.B.A., XII, 1056].

d —— (On the problem of genetic heterogeneity in plant tissues). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V.I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: Nos 5–6: p. 35. [P.B.A., XV, 1050].

Gorškov, I. and Jakovlev, P. (The possibilities revealed by the achievements of I.V. Mičurin). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1934: No. 11.

Grebennikov, P. E. (The multiple mass selection as an agrotechnical method of increasing the yield of winter wheat). *Trudy Belaruskaga Seljskagaspadarčaga Instytuta* (*Ann. White Russia Agric. Inst.*) 1939: **10** (32): 1–12. [P.B.A., X, 732].

Gumenjuk, I. P. and Podufaliĭ, T. I. (Methods and achievements of I.V. Mičurin and their utilization in the Ukraine). *Žurnal Institutu Botaniki Akademiĭ Nauk URSR* (*J. Inst. Bot. Acad. Sci. Ukraine*) 1936: No. 10 (18): 161–70. [P.B.A., VIII, 224].

Gurov, P. (The work of Ivan Vladimirovič Mičurin). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1935: **7** (1): 172–85. [P.B.A., VI, 986].

a Hačaturov, S. P. (The inheritance of the capacity for selective fertilization in gametes on fertilization). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 21–24. [P.B.A., XI, 692].

b —— (Differences in the progenies of hybrids). *Jarovizacija* (*Vernalization*) 1940: No. 6 (33): 29–44. [P.B.A., XII, 78].

Hadjinov, *see* Hadžinov.

Hadžinov, M. A. (Selective fertilization in maize when pollinated with a pollen mixture). *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1940: **29**: 410–14. [P.B.A., XI, 699].

a Haldane, J. B. S. *The Marxist Philosophy and the Sciences.* London 1938: Pp. 183.

b —— Lysenko and genetics. *Sci. and Soc.* 1940: **4**: 433–37. [P.B.A., XI, 321].

c —— Heredity, development and infection. *Nature, Lond.* 1944: **154**: p. 429. [P.B.A., XV, 20].

Halifman, I. A. (Darwinian meeting of the Lenin Academy of Agricultural Sciences). *Priroda* (*Nature*) 1940: No. 4: 107–13.

Harečko-Savickaja, H. Selektive Befruchtung. *Z. Pflanzenz.* 1944: **26**: 187–98. [P.B.A., XV, 1338].

Harrington, J. B. Intra-varietal crossing in wheat. *J. Amer. Soc. Agron.* 1944: **36**: 990–91. (Abst.) [P.B.A., XV, 944].

Hazina, E. P. (Alteration of the fruit form in the seed progeny of grafts). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): 84–89. [P.B.A., XII, 84].

Hitrinskiĭ, V. F. (Intravarietal crossing). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: No. 6: 15–18. [P.B.A., VIII, 1105].

Huxley, J. Evolutionary biology and related subjects. *Nature, Lond.* 1945: **156**: 254–56. [P.B.A., XVI, 17].

Iljinskiĭ, A. P. (Ecotypes, polyploids and natural selection). *Priroda* (*Nature*) 1943: No. **1**: 63–67. [P.B.A., XIV, 1138].

Isaev, S. I. (The late I. V. Mičurin's methods as the basis of plant-breeding work). *Za Mičurinskoe Plodovodstvo* (*Horticulture by Mičurin's Methods. Bull. Lenin Acad. Agric. Sci. Res. Inst. Fruit Grow. I. V. Mičurin*) 1936: No. **4**: **7–15**. [P.B.A., VII, 1347].

a Jablokov, A. S. (Differences in the F_1 interspecific hybrids of woody forest trees). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 33–40. [P.B.A., XI, 81].

b —— (Pro Darwinism in breeding woody forest trees). *Lesnoe Hozjaĭstvo* (*Forestry*) 1940: No. 4: 75–77. [P.B.A., XI, 1113].

a Jakovlev, P. (Mičurin and science). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1934: No. 11: 22–26. [P.B.A., V, 773].

b —— [To the memory of I. V. Mičurin (on the fifth anniversary of his death)]. *Plodovo-Jagodnye Kuljtury* (*Fruit Crops*) 1940: No. 4: 3–8. [P.B.A., XIV, 609].

c —— (Along Darwin's great path). *Sadovodstvo* (*Horticulture*) 1940: No. 6: 18–24. [P.B.A., XI, 322].

d —— (Development of I. V. Mičurin's doctrine by the research institutes). *Sady i Ogorody* (*Fruit and Vegetable Gardens*) 1941: No. 6: 2–8. [P.B.A., XII, 572].

e —— (I. V. Mičurin). *Sovetskaja Botanika* (*Soviet Botany*) 1941: No. 1–2: 5–13. [P.B.A., XIII, 1142].

a Jakovuk, A. (The occurrence of interchanged dominance in some tobacco hybrids). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): p. 120. [P.B.A., XII, 1138].

b —— (Selective fertilization in tobacco plants). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1941: No. 4: 3–7. [P.B.A., XII, 210].

c —— (Intravarietal crossing in *Nicotiana Tabacum* L.). *Vsesojuz. Naučno-Issledovateljskiĭ Institut Tabačnoĭ i Mahoročnoĭ Promyšlennosti imeni A. I. Mikojana* (*VITIM*). (*All-Union Mikojan Research Institute of the Tobacco and Makhorka Industry*) *Krasnodar* 1941: No. 143: 29–37. [P.B.A., XV, 676.

a Jakubciner, M. M. (On the pure line). *Jarovizacija* (*Vernalization*) 1935: No. 3: 105–08.

b —— (Once more on the constancy of the pure line). *Jarovizacija* (*Vernalization*) 1936: No. 2–3 (5–6): 19–24.

Jeans, J. *Physics and Philosophy*. Cambridge 1942: Pp. 222.

Jollos, V. Grundbegriffe der Vererbungslehre insbesondere Mutation, Dauermodifikation, Modifikation. *Handbuch der Vererbungswissenschaft* 1939: **1** D: 1–106.

Jones, D. *Selective Fertilization*. Chicago 1928: Pp. 155.

Kamšilov, M. M. (Natural selection as a creative factor of evolution). *Priroda* (*Nature*) 1939: No. 11: 26–34.

Kargopolova, N. N. (Intravarietal crossing and the increase in spring wheats of resistance to smut). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 67–69. [P.B.A., XII, 1044].

Kartman, L. Soviet genetics and the "autonomy of science". *Sci. Mon., N.Y.* 1945: July: 67–70. [P.B.A., XVI, 115].

Keller, B. (Genetics on new principles). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Sci. U.S.S.R.*) 1944: No. 10: 3–10. [P.B.A., XV, 897].

Kesselring, W. Zur Züchtung neuer Obstsorten durch J. W. Mitschurin. Ergebnisse sechzigjähriger Arbeit (1874 bis 1934). *Obst- u. Gemüseb.* 1936: **82**: 67–71. [P.B.A., VII, 1031].

Kobeljkov, V. V. (The effect of free pollination on the yield of buckwheat). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 120–21. [P.B.A., XII, 1057].

Kolj, A. (Reconstruction of plant industry in the U.S.S.R.). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: **10**: 87–107. [P.B.A., VII, 891].

a Kolmogorov, A. N. On a new confirmation of Mendel's laws. *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1940: **27**: 37–41. [P.B.A., XII, 389].

b —— *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1941: **31**: 47–50.

Kolman, E. Is it possible to prove or disprove Mendelism by mathematical and statistical methods? *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1940: **28**: 834–38. [P.B.A. XIII, 1139].

Komarov, V. L. Marx and Engels on biology. *Marxism and Modern Thought*. Trans. R. Fox. London 1935: 190–234.

Kononenko, M. V. (The effect of the dates of seeding on the nature of winter wheat in successive generations). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1942: Nos 1–2: 41–44. [P.B.A., XIV, 509].

Konstantinov, P. N., Lisicyn, P. I. and Kostov, D. (Some remarks on the work of the Odessa Institute of Plant Breeding and Genetics). *Socialističeskaja Rekonstruckcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 11: 121–30; also *Jarovizacija* (*Vernalization*) 1936: No. 5 (8): 15–29. [P.B.A., VII, 1153].

Kopeljkievskiĭ, G. V. and Mihaĭlov, K. N. (Experiments on the vegetative hybridization of buckwheat). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35); 111–14. [P.B.A., XII, 1058].

Kosmodemjjanskiĭ, V. N. (Transgression in different generations of hybrids of *Nicotiana Tabacum*). *Vsesojuz. Naučno-Issledovateljkskiĭ Institut Tabačnoĭ i Mahoročnoĭ Promyšlennosti imeni A. I. Mikojana* (*VITIM*). (*All Union Mikcjan Research Institute of the Tobacco and Makhorka Industry* (*Krasnodar* 1941: No. 143: 3–28. [P.B.A., XV, 677].

Kostjučenko, I. (Winter rye varieties for the east of the U.S.S.R.). *Socialističeskoe Seljskoe Hozjaĭstvo* (*Socialist Agriculture*) *Moscow* 1942: No. 1: 47–50. [P.B.A., XIV, 849].

Kostrjukova, K. Ju. and Černojarov, M. V. (In favour of Mičurin's cytobiology). *Jarovizacija* (*Vernalization*) 1940: No. 3 (30): 140–51. [P.B.A., XI, 620].

Kovalevskaja, L. Ja. (Overcoming the unfavourable influence of inbreeding in cross-pollinated plants). *Ovoščevodstvo* (*Vegetable Growing*) 1940: No. 7: 21–25. [P.B.A., XI, 326].

Kovarskiĭ, A. E. (The process of form development in interspecific hybrids of cowpeas under the influence of different conditions of environment). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1939: Nos. 10–11: 9–13. [P.B.A., X, 913].

Kozlov, M. and Matveev, N. (Is it necessary to regenerate varieties?) *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1935: No. 2 (10): 17–20. [P.B.A., VI, 771].

Kravčenko, S. K. (Questions of intervarietal cross-pollination in maize). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1940: No. 5: 11–13. [P.B.A., XI, 701].

Krenke, N. P. (*The Theory of Cyclic Senescence and Rejuvenation in Plants and its Practical Application*). Moscow 1940: Pp. 135.

Krevčenko, L. E. (A case of heterogeneity in the first hybrid generation). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 111–12. [P.B.A., XII, 1217].

a KROTOV, A. S. (On improving self-pollinated varieties). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1940: No. 1: 22–26. [P.B.A., XI, 349].

b —— (Cross pollination of buckwheat varieties). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: Nos 5–6: 13–15. [P.B.A., XV, 973].

KRUŽILIN, A. S. [The physiology of variation and inheritance in plants (a working hypothesis)]. *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) Saratov 1941: No. 1: 142–49. [P.B.A., XI, 909].

KULJČENKO, N. I. (Intravarietal crossing of tomatoes in the greenhouse). *Ovoščevodstvo* (*Vegetable Growing*) 1939: No. 6: 41–42. [P.B.A., IX, 1647].

LAMARCK, J. B. P. A. *Philosophie Zoologique.* Paris 1809: Pp. 428+475.

LAZAREV, N. (On the method of rejuvenation of varieties according to "marriage for love"). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1936: No. 4: 26–27. [P.B.A., VII, 517].

LENIN, V. I. *Materialism and Empiriocriticism.* Trans. A. Fineberg. London 1938: Pp. 384.

LESIK, I. L. (Capacity for selective fertilization in maize). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 105–06. [P.B.A., XI, 700].

LEVOŠIN, V. K. (Form development within a clone in apples). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialist Grain Farming*) Saratov 1940: No. 1: 142–58. [P.B.A., XII, 238].

LEVY, H. A scientific worker looks at dialectical materialism. *Aspects of Dialectical Materialism.* London 1934: 1–30.

LEWICKI, S. O niektórych szkodliwych prądach rosyjskiej genetyki i hodowli roślin. *Przegl. Doświad. Roln.* 1938: **1**: 261–65. [P.B.A., IX, 1361].

a LYSENKO, T. D. (The physiology of plant development in breeding work). *Semenovodstvo* (*Seed Growing*) 1934: No. 2: 20–31. [P.B.A., V, 638].

b —— (The organization of seed production). *Jarovizacija* (*Vernalization*) 1935: No. 1: 25–64.

c —— (Reply to the article "Some Critical Remarks" by G. K. Meĭster). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1935: No. 2 (10): 21–30. [P.B.A., VI, 765].

d —— (How we are fulfilling our obligation). *Bjulletenj Vsesojuz. Akad. S.-H. Nauk im. V. I. Lenina* (*Bull. Lenin Acad. Agric. Sci.*) 1936: No. 11: 5–9. [P.B.A., VII, 1164].

e —— (Making up a lost year). *Jarovizacija* (*Vernalization*) 1936: Nos 2–3 (5–6): 9–18.

f —— (The first results). *Jarovizacija* (*Vernalization*) 1936: No. 5 (8): 3–14.

g —— (Do they confound or are they confounded). *Jarovizacija* (*Vernalization*) 1936: No. 5 (8): 30–44.

h —— (Intravarietal crossing of self-pollinated plants). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: **10**: 70–86. [P.B.A., VII, 897].

i —— (Reply to the article "Some Remarks on the Odessa Institute of Plant Breeding and Genetics" by P. N. Konstantinov, P. I. Lisicyn and Dontčo Kostov). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 11: 131–38. [P.B.A., VII, 1154].

j —— (On two directions in genetics). *Jarovizacija* (*Vernalization*) 1937: No. 1 (10): 29–75; also *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 2: 50–74 and *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: **2**: 21–24.

k LYSENKO, T. D. (About what "conclusions" does Academician Konstantinov worry himself?) *Jarovizacija* (*Vernalization*) 1937: No. 2 (11): 19–31.

l —— (Intravarietal crossing of self-pollinated plants). *Novoe v Seljskom Hozjaĭstve* (*What is New in Agriculture*) Moscow 1937: No. 1: Pp. 48.

m —— (Intravarietal crossing and the Mendelian "law" of segregation). *Jarovizacija* (*Vernalization*) 1938: Nos 1–2 (16–17): 114–26.

n —— (Intravarietal crossing and the Mendelian law of segregation). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1938: Nos 8–9: 1–6. [P.B.A., IX, 642].

o —— (Methods of producing winter-hardy varieties of winter crops in the East). *Jarovizacija* (*Vernalization*) 1939: No. 2 (23): 55–60.

p —— In response to an article by A. N. Kolmogoroff. *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1940: **28**: 832–33. [P.B.A., XIII, 1139].

q —— (The ways of controlling plant organisms). *Jarovizacija* (*Vernalization*) 1940: No. 3 (30): 27–44. [P.B.A., XI, 622].

r —— (What is Mičurin's genetics?) *Jarovizacija* (*Vernalization*) 1940: No. 6 (33): 3–19. [P.B.A., XII, 85].

s —— (About some of the main aims of agricultural science). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1942: Nos 5–6: 3–12. [P.B.A., XIV, 482].

t —— (On inheritance and its variability). *Socialističeskoe Seljskoe Hozjaĭstvo* (*Socialistic Agriculture*) Moscow 1943: Nos 1–2: 47–69; Nos 3–4: 36–51. [P.B.A., XV, 117].

LYSENKO, T. D. and DOLGUŠIN, D. A. (Intravarietal crossing of winter and spring wheat). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1938: Nos 8–9: 7–8. [P.B.A., IX, 643].

LYSENKO, T. D. and PREZENT, I. I. (*Plant breeding and the theory of phasic development*). Moscow 1935: Pp. 64.

a MAKSIMOVIČ, M. M. (On new ways of breeding potatoes). *Plodoovoščnoe Hozjaĭstvo* (*Fruit and Vegetable Growing*) 1938: No. 12: 5–7. [P.B.A., IX, 1093].

b —— (New methods of breeding work with potato). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 1: 10–14. [P.B.A., XIII, 541].

MALAHOVSKIĬ, A. (On the biological characteristics of intraracial crossing). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 2: 96–103. [P.B.A., VIII, 1101].

MALJCEV, T. S. (Paper read at the Narkomzem board meeting). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): p. 148. [P.B.A., XII, 1041].

MANGELSDORF, P. C. Genetics in Russia. *Chronica Botanica* 1941: **6**: 256–57. [P.B.A., XI, 618].

MARX, K. *Das Kapital.* Hamburg 1885: Pp. 808+526.

MAŠTALER, G. A. (Darwin and genetics). *Priroda* (*Nature*) 1939: No. 11: 42–50.

a MATHER, K. Variation and selection of polygenic characters. *J. Genet.* 1941: **41**: 159–93. [P.B.A., XI, 252].

b —— Genetics and the Russian controversy. *Nature, Lond.* 1942: **149**: 427–30. [P.B.A., XII, 646].

c —— Polygenic inheritance and natural selection. *Biol. Rev.* 1943: **18**: 32–64. [P.B.A., XIII, 378].

MAXIMOV, N. A. (The development and achievements of plant physiology in the U.S.S.R. during 25 years). *Uspehi Sovremennoĭ Biologii* (*Advances in Modern Biology*) 1943: **26**: 469–98.

MEDVEDEVA, G. (The problem of dominance in wheat). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: Nos 8–9: 29–36. [P.B.A., XV, 941].

MEĬSTER, G. (Some critical remarks). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1935: No. 2/10: 13–17. [P.B.A., VI, 765].

—— (Summary of the discussion on genetics and breeding). *Bjulletenj Vsesojuz. Akad. S.-H. Nauk im. V. I. Lenina* (*Bull. Lenin Acad. Agric. Sci.*) 1937: No. 1: 4–19. [P.B.A., VIII, 66].

MELJNIČENKO, JA. S. and TREGUBENKO, M. JA. (Effectiveness of intravarietal crossing in winter wheat). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1940: No. 10: 9–13. [P.B.A., XI, 940].

MICHURIN, *see* MIČURIN.

a MIČURIN, I. V. (*Results of Sixty Years' Work*). Moscow 1936: Pp. 492.

b —— (The inapplicability of Mendel's Law to hybridization). *Selected Works, Voronež* 1939: 204–08.

c —— (The use of mentors and the rearing of hybrid seedlings and examples of marked changes in varieties of fruit trees under the influence of various subsidiary factors). *Selected Works, Voronež* 1939: 209–23.

a MILLARDET, A. Note sur l'hybridation sans croisement ou fausse hybridation. *Mem. Soc. Sci. Phys. Nat. Bordeaux* 1894: 4me Sér.: **4**: 347–72.

b —— Note sur la fausse hybridation chez les ampelideés. *Rev. Vitic., Paris* 1901: **16**: 677–80.

a MISES, R. v. *Wahrscheinlichkeit, Statistik und Wahrheit.* Vienna 1936: Pp. 282.

b —— *Wahrscheinlichkeitsrechnung und ihre Anwendung in der Statistik und theoretischen Physik.* Leipzig and Vienna 1931: Pp. 574.

MOGILEVA, A. (Intravarietal crossing in peas). *Jarovizacija* (*Vernalization*) 1941: No. 1 (34): p. 104. [P.B.A., XII, 294].

MORGAN, W. T. J. Transformation of pneumococcal types. *Nature, Lond.* 1944: **153**: 763–64. [P.B.A., XIV, 1063].

MUHIN, N. D. (Variability in the yielding capacity of the seed progeny from different parts of the ear). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36: 104–06. [P.B.A., XII, 982].

MULLER, H. J. (The present state of experimental data on the nature of the gene). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 2: 75–95: also *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: No. 2: 26–28.

MURALOV, A. I. (Summary of the 4th session). *Bjulletenj Vsesojuz. Akad. S.-H. Nauk im. V. I. Lenina* (*Bull. Lenin Acad. Agric. Sci.*) 1937: No. 1: 1-3. [P.B.A., VIII, 67].

MUSIĬKO, A. S. (Supplementary pollination—a method of increasing the yield of farm crops, and improving their seed types). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 56–62. [P.B.A., XII, 974].

NAUDIN, C. Nouvelles recherches sur l'hybridité dans les végétaux. *Ann. Sci. Nat. Bot.* 1863: 4me Sér.: **19**, 180–203.

NAZARENKO, S. I. (Drought resistance of plants as influenced by the conditions under which the seeds were raised). *Sovetskaja Botanika* (*Soviet Botany*) 1941: No. 1–2: 72–79. [P.B.A., XIII, 1214].

NEILSON JONES, W. *Plant Chimaeras and Graft Hybrids.* London 1934: Pp. 136.

NOVIKOV, V. V. (Pea-bean hybrids). *Jarovizacija* (*Vernalization*) 1940: No. 3 (30): 106–18. [P.B.A., XI, 835].

ODINCOV, V. A. (Mičurin and his work). *Za Mičurinskoe Plodovodstvo* (*Horticulture by Mičurin's methods. Bull. Lenin Acad. Agric., Sci. Res. Inst. Fruit Grow. I. V. Mičurin*) 1934: No. 3: 5–11. [P.B.A., V, 1101].

OETTINGEN, H. VON. Die Zuchtmethoden I. W. Mitschurins. *Pflanzenbau* 1936: **12**: 477–82. [P.B.A., VII, 271].

ORBELI, L. A. (Development of biological sciences in the U.S.S.R. during the last 25 years). *Jubilee Session of the Academy of Sciences of the U.S.S.R. Moscow–Leningrad* 1943: 216–27. [P.B.A., XIV, 1117].

PINCUS, J. W. The genetic front in the U.S.S.R. *J. Hered.* 1940: **31**: 165–68. [P.B.A., X, 668].

PISAREV, W. E. and VINOGRADOVA, N. M. Hybrids between wheat and *Elymus*. *C.R.* (*Doklady*) *Acad. Sci. U.R.S.S.* 1944: **45**: 129–32. [P.B.A., XV, 947].

PJATNICKIĬ, S. S. (Hybridization in oaks). *Lesnoe Hozjaĭstvo* (*Forestry*) 1939: No. 7: 38–43. [P.B.A., X, 880].

PLATIKANOV, N. (The foundations of modern genetics are questioned). *Zemledelie, Sofia* 1937: **41**: 117–20. [P.B.A., VIII, 1107].

PLOTNIKOV, I. G. (Vegetative hybridization of cereals and its importance in breeding work). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 6: 69–88. [P.B.A., XII, 425].

POLANYI, M. The autonomy of science. *Sci. Monthly* 1945: 141–50.

POLJANSKIĬ, V. I. [Anti-Darwinism in the native country of Darwin. (The recent work of Prof. Punnett)]. *Priroda* (*Nature*) 1939: No. **11**: 87–96.

PONOMARENKO, F. A. (The role of selective fertilization in the winter hardiness of the Siberian wheat Skala. *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1941: No. 1: 10–19.

POPOV, G. I. (Constructive selection). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1940: No. 5: 9–11. [P.B.A., XI, 717].

a POTAŠNIKOVA, B. G. (The effect of free intra- and inter-varietal fertilization, and its causes). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 98–100. [P.B.A., XII, 972].

b —— (Promoting early ripening and increasing the yield of the seed generation of the tomato grafted on black nightshade). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 114–16. [P.B.A., XII, 1220].

PRENANT, M. *Biology and Marxism.* Trans. C. D. Greaves. London 1938: Pp. xxiii+223.

a PREZENT, I. I. (Darwin and Timirjazev on the biological significance of close breeding). *Jarovizacija* (*Vernalization*) 1935: No. 3: 57–86.

b —— (The biology of plant development and genetics). *Jarovizacija* (*Vernalization*) 1936: No. 1 (4): 1–46.

c —— (Pro Darwinism in genetics). *Jarovizacija* (*Vernalization*) 1936: No. 5 (8): 45–68.

d —— (On "pure science" and a "reflective-dynamical" defence of it). *Jarovizacija* (*Vernalization*) 1936: No. 6 (9): 25–52.

e —— (Against formalism and metaphysics in genetical science). *Jarovizacija* (*Vernalization*) 1937: No. 1 (10): 81–102.

f —— (False scientific theories in genetics). *Jarovizacija* (*Vernalization*) 1939: No. 2 (23): 87–116.

g —— (Selective fertilization). *Jarovizacija* (*Vernalization*) 1940: No. 5 (32): 137–39. [P.B.A., XI, 910].

PUHALJSKAJA, E. P. (Inheritance by the Irlandec pea of characters acquired as a result of growing in situations geographically different). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 104–07. [P.B.A., XII, 1234].

PUHALSKIĬ, A. (The efficiency of some new methods in seed breeding work). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: No. 4: 36–42. [P.B.A., XV, 888].

a RAZUMOV, V. I. (The behaviour of individual plants of a pure line under the influence of different lengths of day). *Jarovizacija* (*Vernalization*) 1937: No. 2 (11): 74–81.

b —— (Vegetative hybrids). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1940: No. 1: p. 104. [P.B.A., XI, 406].

RJABOV, N. M. (Theoretical and practical importance of seed rejuvenation). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1936: No. 3: 48–57. [P.B.A., VII, 517].

RODIČEV, S. A. (On pure lines). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1936: No. 4: 23–25. [P.B.A., VII, 518].

ROŽALIN, L. V. (Degeneration of the potato and the insufficiency of the theory of infection by mosaic virus diseases). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 1: 15–23. [P.B.A., XIII, 550].

RUBCOV, V. (Variability of winter cereals under natural conditions). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 8: 189–90. [P.B.A., VIII, 1136].

RUDNICKIĬ, N. V. and GLUHIH, K. A. (Intervarietal cross-pollination in rye). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 35–42. [P.B.A., XII, 1066].

ŠAKUROV, V. Z. (Intravarietal crossing in peas). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 112–14. [P.B.A., XI, 843].

SAPEHIN, A. (Chromosomes, segregation and hybrid vigour). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 69–77. [P.B.A., VIII, 734].

SASONKINA, M. M. (Intergeneric hybridization of leguminous plants by the method of I. V. Mičurin. Preliminary communication). *Trudy Seljskohozjaĭstvennoĭ Akad. im. K. A. Timirjazeva* (*Proc. K. A. Timirjazev Agric. Acad.*) *Moscow* 1935: **1** (2) 141–56. [P.B.A., VI, 1440].

a SAX, K. Soviet biology. *Science* 1944: **99**: 298–99. [P.B.A., XIV, 1125].

b —— Soviet biology. *Science*, 1946: **102**: p. 649.

ŠČERBINA, D. R. (Effect of intravarietal crossing and conditions of rearing of the spring wheat TZA/32). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 62–64. [P.B.A., XII, 1028].

SCHNEIDERMANN, *see* ŠNAĬDERMAN.

ŠČIBRJA, N. A. [A cross between Jerusalem artichoke (*Helianthus tuberosus*) and sunflower (*Helianthus annuus*)]. *Vestnik Gibridizacii* (*Hybridization*) 1941: No. 1: 66–84. [P.B.A., XII, 1155].

SEREBRJAKOV, C. C. (The "Origin of Species" and the botanical works of Ch. Darwin). *Priroda* (*Nature*) 1939: No. 11: 77–82.

SEREBROVSKIĬ, A. S. *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: **2**: 25–26.

SILANTJEV, I. G. (Mičurin's methods—the principles of fruit breeding). *Semenovodstvo* (*Seed Growing*) 1934: No. 6: 65–67. [P.B.A., V, 1102].

ŠILOVA, S. N. (Intravarietal hybridization in tomatoes). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1941: No. 1: 24–30.

ŠIMANSKIĬ, N. K. (Controlled conversion of the nature of the spring wheat Erythrospermum 1160 into winter wheat). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 25–29. [P.B.A., XI, 672].

a SINSKAJA, E. N. (New tendencies in plant breeding). *Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Lenin Acad. Agric. Sci.*) *Leningrad* 1937: Pp. 56. [P.B.A., VIII, 1441].

b —— (The species problem in modern botanical literature). *Uspehi Sovremennoĭ Biologii* (*Advances in Modern Biology*) 1942: **15**: 326–59. [P.B.A., XV, 554].

SIPKOV, T. P. (Changes in the number of chromosomes of plant organisms). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 93–98. [P.B.A., XII, 989].

SIZOV, I. (Inbreeding and its application to breeding). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 89–97. [P.B.A., VIII, 752].

ŠLYKOV, G. (Genetics and plant introduction). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 9: 99–112. [P.B.A., VII, 895].

SLUDSKIĬ, N. (The theoretical importance of the works of I. V. Mičurin in breeding and genetics). *Za Mičurinskoe Plodovodstvo* (*Horticulture by Mičurin's Methods. Bull. Lenin Acad. Agric., Sci. Res. Inst. Fruit Grow. I. V. Mičurin*) 1936: No. 3: 35–38. [P.B.A., VII, 1033].

ŠMUK, A. (Biochemical changes of grafted plants and plant transplantation as a method to pose and solve physiological and biochemical problems). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1945: Nos. 1–2: 3–13. [P.B.A., XVI, 138].

SNAĬDERMAN, JA. A. (Intercrossing capacity of wheat with *Agropyron* and the fertility of their hybrids in relation to the conditions of development). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 3: 13–33. [P.B.A., XII, 121].

SOLODOVNIKOV, F. S. (Significance of age and origin of grafted potato plants in vegetative hybridization). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 3: 20–27. [P.B.A., XIV, 236].

SOLOVJENA, N. A. (Changes in the chemical composition of tomatoes as a result of grafting). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 109–11. [P.B.A., XII, 1222].

STALIN, I. V. *Leninism*. Trans. London 1940: Pp. 667.

STEPANOV, P. A. (The efficacy of Mičurin's principles). *Sadovodstvo* (*Horticulture*) 1939: No. 12: 22–23. [P.B.A., X, 850].

a ŠULYNDIN, A. F. (On the question of the selection of mother plants in hybridization). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1939: No. 9: 7–10. [P.B.A., X, 393].

b —— (Further notes on differences in first generation hybrids). *Jarovizacija* (*Vernalization*) 1940: No. 4 (31): 104–05. [P.B.A., XI, 658].

c —— (On choosing the mother plant in hybridization). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1940: No. 1: 50–57. [P.B.A., XI, 347].

d —— ("Secondary segregation" in hybrids of hard wheats). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 118–19. [P.B.A., XII, 1025].

e —— (Some examples of dominance and absorption in wheat hybrids). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 119–20. [P.B.A., XII, 1026].

a SVINAREV, V. I. (The segregation of F_2 and F_3 wheat hybrids with respect to the length of the vegetative period). *Izvestija Akad. Nauk S.S.S.R. Serija biologičeskaja.* (*Bull. Acad. Sci. U.R.S.S., Ser. Biol.*) 1942: No. 3: 178–80. [P.B.A., XIII, 1209].

b —— (An investigation of the length of the vegetative period in wheat. On the dominance in F_1). *Izvestija Akad. Nauk S.S.S.R. Serija biologičeskaja.* (*Bull. Acad. Sci. U.R.S.S., Ser. Biol.*) 1942: No. 3: 173–77. [P.B.A., XIII, 1208].

a TATARINCEV, A. S. (An experiment on vegetative rapprochement of the pear and the mountain ash). *Za Mičurinskoe Plodovodstvo* (*Horticulture by Mičurin's Methods. Bull. Lenin Acad. Agric., Sci. Res. Inst. Fruit Grow. I. V. Mičurin*) 1934: No. 1: 16–22. [P.B.A., V, 1106].

b —— (The search for methods of overcoming difficulties in crossing). *Za Mičurinskoe Plodovodstvo* (*Horticulture by Mičurin's Methods. Bull. Lenin Acad. Agric., Sci. Res. Inst. Fruit Grow. I. V. Mičurin*) 1934: No. 3: 31–38. [P.B.A., V, 1103].

TATARINCEV, A. S. and SILANTJEV, I. G. (I. V. Mičurin's relationship to Mendel's "Laws"). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1940: No. 1: 11–14. [P.B.A., X, 678].

TER-AVANESJAN, D. V. (The effectiveness of intra-varietal crosses in cotton). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1941: No. 1: 35–40.

TIMIRJAZEV, K. A. (*The historical method in biology*). Moscow 1943: Pp. 208.

TIMOFEEVA-TJULINA, M. (Training of plants—a potent means of increasing the hardiness of winter wheat). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1940: No. 1: 37–39. [P.B.A., XI, 360].

a TITOV, P. I. (Inheritance of awns in wheat). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 1: 122–41. [P.B.A., XII, 123].

b —— (Inheritance of grain colour in wheat). *Socialističeskoe Zernovoe Hozjaĭstvo* (*Socialistic Grain Farming*) *Saratov* 1940: No. 3: 34–43. [P.B.A., XII, 124].

TROFIMEC, N. H. (Selective fertilization in *Allium fistulosum*). *Vestnik Socialističeskogo Rastenievodstva* (*Soviet Plant Industry Record*) 1941: No. 1: 31–34.

TURBIN, N. V. (Mixed inheritance and somatic segregation in hybrid plants). *Jarovizacija* (*Vernalization*) 1941: No. 2 (35): 85–97. [P.B.A., XII, 978].

TUŠNJAKOVA, M. (Grafting of alkaloid plants). *Doklady Vsesojuz. Akad. Seljsk. Nauk im. V. I. Lenina* (*Proc. Lenin Acad. Agric. Sci. U.S.S.R.*) 1944: No. 10: 24–31. [P.B.A., XV, 1052].

UDOLJSKAJA, N. L. (Conditions of development of seed in the ear, and the process of segregation in spring wheat hybrids). *Jarovizacija* (*Vernalization*) 1941: No. 3 (36): 101–04. [P.B.A., XII, 981].

USATOV, S. P. (Fruit growing in the north). *Plodovo-Jagodnye Kuljtury* (*Fruit Crops*) 1940: No. 4: 80–81. [P.B.A., XIV, 611].

VAHRUŠEV, E. T. (The biological variation of a pure line). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: No. 7: 22–25. [P.B.A., VIII, 1104].

VAKAR, B. (Degeneration of varieties in connexion with self-pollination). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 113–27. [P.B.A., VIII, 753].

a VAVILOV, N. (Scientific methods in Soviet plant industry). *Socialističeskaja Rekonstruckcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 12: 33–46. [P.B.A., VIII, 740].

b —— (Soviet breeding methods). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1937: No. 2: 27–49; also *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: **2**: 18–20. (P.B.A., VIII, 1093].

VENJJAMINOV, A. N. (Improvement of old varieties of fruit trees by selection of vegetative mutations). *Plodovo-Jagodnye Kuljtury* (*Fruit Crops*) 1940: No. 5: 15–20. [P.B.A., XIV, 271].

a VINOGRADOVA, E. I. (Repeated selection within a pure-line variety). *Selekcija i Semenovodstvo* (*Breeding and Seed Growing*) 1937: No. 4: 21–22. [P.B.A., VIII, 754].

b VINOGRADOVA, N. B. (Methods of breeding potatoes for blight resistance). *Vestnik Ovoščevodstvo i Kartofelj* (*Vegetable and Potato Journal*) 1940: No. 3: 39–71. [P.B.A., XIV, 241].

VORONJIK, B. A. (The relationship between agriculturally desirable characters in spring vetch and the date of seed production). *Trudy Zonaljnogo Inst. Zernovogo Hozjaĭstva Raĭonov Nečernozemnoĭ Polosy* (*Trans. Zonal Inst. Grain Husbandry Non-Black-Soil Districts*) 1941: No. 10: 85–91. [P.B.A., XV, 250].

WAGNER, S. Die Übertragung nicht mendelnder Buntblättrigkeit durch den Pollen bei Roggen. *Arch. Klaus-Stift. Vererb-Forsch.* 1942: **17**: 449–53 [P.B.A., XIV, 848].

WEISS, F. E. The problem of graft hybrids and chimaeras. *Biol. Rev.* 1930: **5**: 231–71.

ZAVADOVSKIĬ, M. M. (Against obliquities in the attacks on genetics). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 8: 84–96; also *Jarovizacija* (*Vernalization*) 1936: No. 6 (9): 5–24.

a ŽEBRAK, A. R. (Grafting in genetically different lines of peas). *Trudy Seljskohozjaĭstvennoĭ Akad. im. K. A. Timirjazeva* (*Proc. K. A. Timirjazev Agric. Acad.*)Moscow 1935: **1**: 88–97. [P.B.A., VI, 439].

b —— (Some modern questions on genetics). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 8: 97–122. [P.B.A., VII, 894].

c —— (Categories of genetics in the light of dialectical materialism). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 78–88. [P.B.A., VIII, 741].

d —— (Some results of Session IV of the Lenin Academy of Agricultural Sciences). *Izvestija Akademii Nauk S.S.S.R. Serija biologičeskaja* (*Bull. Acad. Sci. U.R.S.S., Ser. Biol.*) 1937: 671–702. [P.B.A., VIII, 1439].

e —— Soviet biology. *Science* 1945: **102**: 357–58. [P.B.A., XVI, 596].

ZEMIT, V. E. (The influence of wide spacing on the hereditary variation of varieties of fibre flax). *Len i Konoplja* (*Flax and Hemp*) 1940: No. 1: 40–43. [P.B.A., XI, 170].

ŽUKOVSKIĬ, P. (Reply to a critic and remarks on some truly controversial questions). *Socialističeskaja Rekonstrukcija Seljskogo Hozjaĭstva* (*Socialist Reconstruction of Agriculture*) 1936: No. 12: 153–60. [P.B.A., VIII, 742].

PRINTED BY W. HEFFER & SONS LTD., CAMBRIDGE